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CONTENTS

American Veterinary Education and its Problems: PROFESSOR VERANUS A. MOORE 457

The British Association for the Advancement of Science:—
Address to the Botanical Section: PROFESSOR F. E. WEISS 464

Work at the Marine Biological Station at San Diego: PROFESSOR WILLIAM E. RITTER 480

The International Eugenics Congress 483

Professor Johannsen's Columbia Lectures ... 484

Scientific Notes and News 484

University and Educational News 487

Discussion and Correspondence:—
The Cotton Worm in Massachusetts: PROFESSOR H. T. FERNALD. *Vectorial Treatment of Secondary Maxima in Grating Spectra:* PROFESSOR HENRY CREW 488

Quotations:—
The University President and his Professors 488

Scientific Books:—
De Vries's The Mutation Theory: DR. C. STUART GAGER. *Walker's Outlines of the Theory of Electromagnetism:* PROFESSOR E. P. ADAMS 491

Contagious Abortion in Cattle: PROFESSOR H. L. RUSSELL 494

Special Articles:—
A Counting Method for the Mechanical Analysis of Soils: C. C. FLETCHER. *A Posterior Communication of the Air-bladder with the Exterior in Fishes:* PROFESSOR EDWIN CHAPIN STARKS 495

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AMERICAN VETERINARY EDUCATION AND ITS PROBLEMS[1]

IN analyzing the subject assigned as my part of the committee's report, I became deeply impressed with the responsibility that rests upon those who are in a measure charged with the difficult task of molding a system of education for these great countries that will yield efficient veterinary service. The first generation of veterinary educators in America is rapidly passing away. Its efforts were spent in meeting the demands and grappling with the conditions of a new country and there was little time for it to reckon with the educational methods as they had developed in more mature and cultured Europe. While this first generation manfully battled against the onslaught of disease in the rapidly increasing animal population, discoveries were being made, and methods tested and put into operation, pertaining to veterinary education and practise, of which these new countries were unmindful. At no time in the history of man has a generation witnessed greater revolutions in the theories, facts and methods of a profession than those which occurred in rapid succession in the active lifetime of Alexander Liautard, Andrew Smith, Duncan McEachran and James Law, three of whom still remain as wise counselors among us. These distinguished men have witnessed all that has been accomplished in the acquisition of definite knowledge of specific diseases, sanitary science, the introduction of the newer

[1] A paper presented by the chairman of the Committee on Intelligence and Education of the American Veterinary Medical Association, at its annual meeting, Toronto, August, 1911.

pathology and therapeutics and the development of our veterinary schools and colleges. It is well for us of the present to recognize that these great leaders saw, as did Lowell, that "New times demand new manners and new men and new conditions demand new schools." Law was the first to carry this idea into practise. Smith followed by affiliating with a great university the school brought into form by his own efforts. McEachran, finding it impossible to provide adequate equipment with the financial support forthcoming, closed the doors of his institution, and Liautard is constantly ringing the call for better and more efficient education.

Standing as it were at the close of the first generation and at the beginning of the second, it seems fitting that this association should devote a little time to the consideration of veterinary education in these countries—what it is, its weaknesses, its strength, its opportunities and above all its responsibilities.

It is a reasonable hypothesis that education directed toward the preparation of men for the practise of veterinary medicine does not differ in principle from that for other professions. The laws of educational evolution therefore apply here as elsewhere, and the acquisition of knowledge that goes to make up a so-called learned profession holds exactly the same position here as it does in human medicine, theology or law. The purpose of education is to engender thought, to eliminate dogma, to enthrone facts and laws and to endow one with an intellectual liberty. Accompanying the possession of definite knowledge there is a corresponding moral obligation to use this knowledge for the benefit of mankind. This is especially true with the medical professions.

If one analyzes the present status of veterinary education in America, measured by the course of general educational laws, it will be found that there still remain in our system or systems of instruction many examples of the methods of the unskilled, remnants of faith in the magic power of the by-gone mystic wand, lingering beliefs in the technical ability of the so-called practical man, as well as a powerful undercurrent of forces demanding education for efficiency. This demand for better training must eventually here, as in other fields, cause ignorance to be replaced by knowledge, unfounded opinions to give way to facts and finally banish forever that error in our teaching which assumes that application can go ahead of the knowledge to be applied. We hear much of veterinary science, forgetting that the application of veterinary medicine is an art based upon a well-defined group of sciences, and that, other things being equal, the success of the artist depends upon his knowledge of the sciences upon which the art rests. This is illustrated by the universal fact that in those countries where the most training in the basic sciences is given and demanded practise is correspondingly most successful. In our systems too much emphasis has been and still is placed upon the diploma and all too little upon what it should represent, thus encouraging the erroneous assumption that it is possible to use what one does not possess.

In order to estimate the true worth of our systems of veterinary education, let us in our imagination strike from our knowledge and from the literature the achievements of the devotees of pure science and then ask ourselves what American veterinary education in itself has done to advance specific knowledge of the nature, treatment and prevention of animal diseases. The honest answer to this question would show that much we are wont to claim for our humane profession comes to

us as a heritage from the great leaders in chemistry, physiology and pathology. The analyst finds in many of our systems the presumptuous effort to correlate the basic sciences into a workable unit in men who do not possess the first principles of chemistry, physics and biology and to instruct such undisciplined men in the many complicated topics that must find place in a modern veterinary curriculum in the brief period of a few short months. While perhaps in earlier years, before there were public schools, and when our curricula were less crowded, these methods met the requirements, the time has come when they remind one of the legend of the clock. It is said that in one of the old towers of Europe there was a clock. As time went on the hands dropped off and the figures on the dial became obliterated; but the sexton, grown old in service, ascended the ladder every week and wound the slowly rusting spring and the clock kept ticking, ticking, while all those who came to learn the time of day went away disappointed, for it told them nothing. A like experience, as proved by many statements from agriculturists, is all too often recorded by those who, in trying to save their suffering animals, have witnessed the hopeless efforts of men who have mistaken diplomas for knowledge and who have sought to be practical without possessing the knowledge with which to aid the sick individual to set aright the disharmonies in the physical body.

Although it is easier to tear down than to build up, it is not my purpose to dwell upon the things we wish were not, but rather to point to the broad foundations that have been and are being laid and to the towers of strength and service that sooner or later must rise upon these foundations. The first and most important stone that is being placed in the foundation for a better and more efficient veterinary education is the training of the owners of animals in the basic sciences upon which veterinary art itself is founded. The teaching in chemistry, botany, physiology and bacteriology given in our agricultural colleges is removing from the minds of men the mask concerning the nature of disease and its treatment that superstition has long held in place. The truth uttered by John Hunter centuries ago that diseases should be studied as objects of natural history has been accepted. It is now recognized that when there is a disability of the body or a morbid process there is some physical cause; and that the remedy lies in rendering the inflicted individual some definite assistance to the methods nature herself has provided to defend the body against such irritants, whatever they are, or to heal the injuries produced. The principles of immunity as laid down and demonstrated by Metchnikoff and Ehrlich are being outlined in readers for pupils in the common schools. The U. S. Bureau of Animal Industry, experiment stations and agricultural colleges are popularizing technical knowledge and sending it broadcast throughout the country in bulletins and circulars so that those who escape the college curriculum are caught in the coils of these popular mechanisms for instruction. With a clientele versed in the very sciences that must be applied by the veterinarian, can a practitioner hope for success, or even for a chance of success, if he himself is not in possession of a still greater knowledge of these same subjects?

A second stratum in our foundation is the gradual differentiation of a group of sciences and their special development for the purpose of promoting the practise of veterinary medicine. Until recently, and in certain places, this still obtains, there has been the tendency to instruct students of

agriculture and others in the treatment of animal diseases. "Every man his own veterinarian" has been the slogan of many, and because of the scarcity of properly trained practitioners it seemed for a time to be the only source of relief. Gradually the line of demarcation is appearing between the general scientific knowledge of the layman and that required for the successful practise of veterinary medicine. Further, as people become versed in the biological sciences, they are better qualified to judge of scientific work; but nowhere in general education are men trained in the details of fact and physical law necessary to wrestle with the diagnosis and treatment of animal diseases. The holding up of a professional standard is more and more in evidence. The aphorism that "the treatment of a disease should not be given to a man who can not make the diagnosis" is prevailing more than ever before, and with it great suffering to dumb creation is being avoided.

Nowhere in educational extension work is there more danger than in the effort to popularize knowledge which can not be popularized. Some years since I was asked to prepare an article for a great agricultural encyclopedia on the treatment of all of the diseases of animals. My reply was that I could not do it for two reasons, first, I did not possess the necessary knowledge and, secondly, I would not if I could. Whatever our duties may be, they certainly do not lead us into acts that will encourage uninformed people to interfere with the natural resisting and healing forces of the physical body by the improper application of drugs. As sacred as the Hippocratic oath is the therapeutic axiom, "If we do no good be sure we do no harm." The assumption that a diagnosis can be made by any intelligent individual and that he can apply remedies with hope of success is no longer entertained. The recognition of the line of cleavage between knowledge that can be imparted to and used by the layman and that which can be used effectively only with the full complement of facts which are possessed by the properly trained professional man, is a signal for still better and more lasting achievements.

A third element of strength is the recognition by the existing veterinary schools and colleges that better and more efficient work is demanded of them. Already there is a wide-spread effort to bring about better conditions. As the foundation for a better system of veterinary education has been laid by forces operating largely outside of the profession, it remains for us to build upon this foundation an educational structure adequate to the demands. Many efforts of a more or less spasmodic nature have been put forth to accomplish this. Numerous formulæ have been proposed for the conduct of veterinary schools and colleges. While opinions may differ as to the effectiveness of the various remedies proposed, we may with fairness to all pass from a prescribed formula to the facts and principles that must be reckoned with in securing the desired results. Here we do not differ from human medicine, engineering, agriculture or specialization in any of the sciences. The facts to be considered pertain to the changes in curriculum, methods of teaching and the necessary cost of instruction. The underlying principle is that which governs the growth of knowledge and the discipline of the individual acquiring it.

In former times students acquired their professional knowledge from their preceptors. Later this system obtained in schools where the master in the form of didactic lectures given with more or less ceremony imparted facts considered to be sufficient

for the practitioner. A time came, however, when many discoveries in nature's processes were made and great leaders arose. Those leaders precipitated, as it were, long suspended and hitherto unperceived elements of knowledge so that orderly truth seemed to crystallize in the twinkling of an eye out of what had appeared hitherto but a cloudy mass of facts. Men of this class are not born in every generation; in none are they numerous. Theirs are the master minds. In a strict sense these men are pupils of no masters, but at first they stand alone in expounding new theories. Such men as Virchow, Darwin, Pasteur, Metchnikoff and Ehrlich laid the foundation of rational medicine and brought into the curriculum the consideration of a vast number of topics unknown to their predecessors. These subjects could not be taught by word of mouth only, and consequently laboratories with delicate and expensive apparatus supplemented lectures, and clinics with a great array of instruments of precision were substituted for cases in private practise. Again, to understand the meaning and to profit by these aids the student must be prepared by a training in and a knowledge of the basic sciences. These changes have come about in less than a half century and the lives of men now living span this great epoch-making period.

With the introduction of new subjects and new methods of teaching, the cost of instruction increased. The schools without the equipment can not meet their obligations, nor can they procure the equipment and provide the instruction with the fees that students pay. This is an important fact in higher and professional education everywhere. Many elements enter into this increased cost, the most important of which are expensive equipment and the inability to teach large classes or sections of students and the consequent necessity of providing for units of small numbers. The budgets of our large universities show that it actually costs for every student from two to three times as much as the highest tuition charged, and when the maximum efficiency is attained the expense will be much greater than at present. Recently the president of one of our best technical universities told me that it cost them $450 a year for every man they graduated. It costs our best medical schools from $500 to $1,000 and some of them more a year for every student, and no thoughtful person will assert that efficient veterinary education will cost much if any less. The college which I have the honor to represent expends over $300 annually for every student in addition to the cost of instruction in histology, embryology, chemistry and animal husbandry given by the university, and even with this outlay my heart and head are sorely troubled to satisfy the reasonable demands of the faculty for assistance and equipment with the funds available.

The difficulties involve not only questions of financial support, but also efficient methods for teaching the newer subjects. It often seems that in the development of efficient educational systems the greatest difficulty is the formulation of methods and the enlistment of suitable men for teachers. Some years ago I accepted a position carrying with it the responsibility of teaching pathology and bacteriology in a veterinary college. I went from a research laboratory where I had grown up with the technique and knowledge of certain phases of the subjects. I labored, as have many others, under the delusion that the essential elements could be easily taught. There was no difficulty in securing the interest of the students, but the pangs of disappointment were mine when these same interested men

attempted to apply the knowledge I supposed they possessed, in the actual practise of their profession. Their errors were not more grievous than those of other men, but the things they did and the kind of assistance they sought pointed clearly to a lack of knowledge or understanding at least of the subjects which I believed I had taught with great clearness. This experience has caused me to question the efficiency of many pedagogical methods and to test as best I could different systems of instruction in my own department. The conclusion that seems to be inevitable from the experience of the past is that for the best results the courses in a veterinary curriculum must be dominated by a scientific system of presentation and that the technique, facts and laws of chemistry, anatomy, physiology, bacteriology, pathology, medicine and surgery must be clearly developed before stress can be placed upon the value of fragmentary facts. The difficulty rests not alone in a lack of the knowledge of technique, but quite as much in the inability to apply the principles and to interpret the findings. If veterinary medicine is to benefit the public as it should, it is important that those responsible for the training of men who are to apply the knowledge in practise take fully into account the nature of their teaching. Wherever the true scientific spirit dominates the final results prove to be most helpful. It is not my purpose, nor do I believe it is possible, to outline a schedule to be followed by all. It is, however, within our power to give to veterinary medicine the dignity commensurate with its economic and vital relations to the live-stock interests of the country and to teach it as a group of correlated sciences and not as an aggregation of disconnected facts.

During the last quarter of a century a great revolution has taken place in matters educational, and the period of readjustment is upon us. The problem as presented to us by the rapid bursting forth of enormous numbers of facts and new methods is easily stated. How can young men be fitted for their profession for $125 a year when it actually costs from three to four times as much to provide the necessary instruction? This is a problem that almost chills our enthusiasm. It is so sudden, so contrary to precedent, so unreasonable from our accustomed point of view, that there is an inclination to dispute it. However, as science can not rise above natural law but must work through it, so veterinary education can not depart from the laws that govern higher education, but must follow them. If the veterinary profession attains its rightful place among the learned professions it must, like the others, adjust itself to the requirements of the times. We are engaged in professional work, not selfish enterprise. The same problem confronts human medicine, and our deliverance is equally as hopeful as that of the sister profession.

Those who have sought for a remedy have found but two solutions to the perplexing problem, namely, the endowment of our schools, either directly or by affiliation with universities or by state assistance. In the United States and Canada private endowments for veterinary schools are not numerous. If the wealthy are not disposed to aid this cause, why can not the members of the profession itself form active alumni associations for the purpose of raising funds to make it possible for their own schools to obtain the necessary financial support? Such a scheme is already in operation in several of our largest universities, and I see no reason why it should not work with veterinary colleges. So long as satisfactory technical education can not be obtained for the fees which most stu-

dents can pay, it is fair that, after receiving the benefit of such training, the alumni should respond willingly and generously. Because such an education is expensive, and because through the wisdom and generosity of certain people provision has been made that boys with little or no money can obtain it, there is no reason why the recipients should not repay the institution for what they have already received.

On the continent of Europe government control has long since solved this difficulty. In America the states and governments are beginning to recognize their responsibility in this matter, but nowhere to the extent the work demands. However, the sentiment is right, and our legislators are waiting for veterinarians to point out the needs, and indicate the course to be followed. I do not know of any line of educational effort that has been more effective in bringing to its aid state assistance than the veterinary cause. There is every reason to expect that when the veterinary profession shows the owners of the more than $4,000,000,000 worth of live stock in the United States alone what can be done to save annually the millions of loss from disease, and when we demonstrate to the health authorities and the public the sanitary importance of a veterinary training, this profession will receive its just recognition. The opportunities for the veterinarian to serve the people are so great that when the profession fully meets its responsibilities, and when leaders appear to solve the problems still waiting solution, veterinary education will become the most idealistic and realistic of the learned professions, and the funds necessary to provide such service will be forthcoming. The need of the time is educational leaders to formulate systems and to develop the many avenues through which we can render service to our clients, our state and our nation. As devotees of veterinary science we yield precedence to none in honesty and lawfulness of purpose or faithfulness of service in the bitter conflict humanity has ever waged and ever must wage against pain and disease.

As already stated, our difficulties are not restricted to finances. There are perplexing pedagogical tasks before us. In every transition period there are dangers often overlooked until it is too late. Medical instruction and practise are changing. The new theories, diagnostic aids and therapeutic agents fairly glisten with possible victories. The recent graduates going out with this new armor are like the unhappy knights of old in the search for the Holy Grail in that they feel their zeal and power to be sufficient to overcome all obstacles. The pendulum of knowledge, like that which beats time, swings from one extreme to the other. Specific knowledge of etiology, the reaction of the tissues, the definite laboratory methods for diagnosis, are accompanied by dangers, for as yet we see only in part. Already the experience with tuberculin, the agglutination test and the reliance on certain findings in blood examinations, point to the fact, which must not be overlooked, that in all of the so-called methods of precision there are sharply defined limitations which sooner or later will be revealed. Again there is in evidence the danger of superficial training in the practical branches, such as theory and practise, because of a dependence on and an over-confidence in laboratory findings or the newer remedies. All too frequently we receive at our laboratory a piece of hide, a bit of intestine or a smear from a liver, sent that we may diagnose the disease from which the animal died. In rare instances this can be done, but it should be clearly understood that the only thing laboratory methods can do is to enable one to see deeper into the condition

than he otherwise could. If the trouble is in the bone, the nervous system or the kidney, a piece of the stomach usually tells us nothing. The microscope or the culture tube can show us only what is present and for assistance they must be used in conjunction with other information or to verify or refute suspicion. This means, therefore, that men who are to become practitioners must be trained in the knowledge of specific etiology, the newer pathology and therapeutics, *in addition to and not as a substitute* for the training in thorough systems of physical examination and in close observation of signs and symptoms in diagnosis and treatment. This not only increases the responsibilities of our teachers, but also demands that technical professional knowledge shall be grafted upon well-trained and noble men. There is no other profession where accuracy, correct interpretation and application are of greater significance than they are in this, yet there is no other where men are elevated to positions of responsibility with so little real preparation. We seem to have forgotten that in the acquisition of knowledge and the coming into an understanding of a profession, the element of time and the discipline of routine work and research are as essential as they are in a science like chemistry or physics.

I have dwelt somewhat at length upon the difficulties in bringing into action men who will meet the demands, improve the opportunities and fulfill the responsibilities of the veterinary profession. If these difficulties are analyzed, it will be found that they are temporary and incident to the transition stage of our knowledge, methods of instruction and the newer conception of the important work of the veterinarian. These all point clearly to the opportunity for veterinary service equal to that of any other occupation or profession. The watchword of to-day is the prevention of disease quite as much as its cure and when this dual purpose becomes fully recognized the necessary means will be forthcoming. While there is much to be accomplished, while our problems are difficult and our burdens sometimes seem greater than we can bear, my voice has proved a recreant servant if any tones of doubt, or fear of ultimate victory, have marred this discussion. There are many reasons to believe that the time will soon come when there will be an American system of veterinary education, laws and practise that will take first place in the world-wide effort to secure the highest possible efficiency in veterinary service.

Veranus A. Moore

THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

ADDRESS TO THE BOTANICAL SECTION[1]

Greatly as I prize the honor done me by the Council of the British Association in electing me to the office of President of the Botanical Section, my gratification has been heightened by the knowledge that the meetings of this section would be graced by the presence of the distinguished group of Continental and American botanists who have just taken part in the International Phytogeographical Excursion to the British Isles.

I am sure that I am voicing the unanimous feeling of the section in offering them a hearty welcome to our deliberations, and, in conveying to them our sense of the honor they have done us by their acceptance of the invitation of this association, I would like to express our hope that by their participation in our proceedings they will help us to promote the advancement of botanical science, for which purpose we are met together.

[1] Portsmouth, 1911.

In view of these special circumstances under which we forgather, it may seem inappropriate if I deal, as I shall be doing, in my presidential address mainly with fossil plants, with the study of which I have been for some time occupied; but I need hardly assure our visitors that, while we entertain some feelings of satisfaction at the contributions made during the past half century towards our knowledge of extinct flora of Britain, yet, as the later sittings of this section will show, and as they have no doubt realized during their peregrinations through this country, our botanical sympathies and energies are by no means limited to this branch of botanical study. Moreover, I hope during the course of my address to point out the ecological interest which is afforded by certain aspects of paleobotany.

On the sure foundations laid by my revered predecessor, the late Professor Williamson, so vast a superstructure has been erected by the active work of numerous investigators that I must limit myself in this address to exploring only certain of its recesses, and I shall consequently confine myself to some aspects of paleobotany which have either not been dealt with in those able expositions of the subject given to this section by previous occupants of this presidential chair, or which may be said to have passed since then into a period of mutation.

The great attractiveness of paleobotany, and the very general interest which has been evinced in botanical circles in the progress of recent investigations into the structure of fossil plants, are due to the light they have thrown upon the relationship and the evolution of various groups of existing plants. It was the lasting achievement of Williamson to have shown, with the active cooperation of many working-men naturalists from the Lancashire and Yorkshire coal-fields, that the structure of the coal-measure plants from these districts can be studied in microscopic preparations as effectively as has been the case with recent plants since the days of Grew and Malpighi. Indeed, had Sachs lived to continue his marvelous historical account of the rise of botanical knowledge up to the years 1880 or 1890, he would undoubtedly have drawn attention to the remarkable growth of our knowledge of extinct plants gained by Binney and Williamson from the plant remains in the calcareous nodules of English coal-seams, and by Renault from the siliceous pebbles of Autun. We are not likely to forget the pioneer work of these veterans, though since then investigations of similar concretions from the coal deposits of this and other countries have been undertaken by numerous workers and have revealed further secrets from that vast store of information which lies buried at our feet.

The possibilities of impression material had indeed been practically exhausted in 1870, and further advance could only come from new methods of attacking the problems that still remained to be solved. The most striking recent instance of the insufficiency of the evidence of external features alone was Professor Oliver's demonstration of the seed-bearing nature of certain fern-like plants, based on microscopical comparison of the structure of the cupule of *Lagenostoma,* with the fronds of *Lyginodendron,* after which discovery confirmatory evidence speedily came to hand from numerous plant impressions examined by Kidston, Zeiller, and other observers.

Undoubtedly in the hands of a less competent and far-sighted observer than Williamson, the new means of investigation might have proved as misleading as the old method had been in many instances. Indeed, as is well known, the recognition

in the sections of *Calamites* and *Sigillarias* of the presence of secondary wood had caused Brongniart to place these plants among conifers, owing to his belief that no vascular cryptogams exhibited exogenous growth in thickness. It required all Williamson's eloquence and pugnacity to convert both British and French paleobotanists to his views, ultimately accepted with such handsome acknowledgment by *Grand' Eury*, one of his antagonists, in his "Géologie et Paleontologie du Bassin Houiller du Gard."

It is curious that Grand' Eury refers in his introduction to the discovery of traces of secondary growth in *Ophioglossum*, and not to that of *Isoëtes*, a plant much more nearly related, as we now believe, to the Lepidodendraceæ, and the structure of which had been so thoroughly investigated by Hofmeister. Williamson, it is true, refers to the secondary growth in the stem of *Isoëtes* in his memoir on *Stigmaria*, but compares it with the periderm-forming cambium of that plant, and does not therefore recognize any agreement in the secondary growth of these two plants.

Adopting von Mohl's interpretation of the root-bearing base of the *Isoëtes* plant as a "caudex descendens," Williamson instituted a morphological comparison between the latter and the branching *Stigmaria*, and came to the conclusion that they were homologous structures, a view which, as we heard at Sheffield, is supported by Dr. Lang on the strength of a reexamination of the anatomy of the stock of *Isoëtes*. If we do not accept Williamson's interpretation of the Stigmarian axis as a downward prolongation of caulome nature, the question remains open whether this underground structure represented a leafless modification of a normal leaf-bearing axis as is known in the leafless rhizomes of *Neottia* and other saprophytic plants, or whether the Stigmarian axes were morphological entities of peculiar character. Grand' Eury, in comparing them with the rhizomes of *Psilotum*, accepted the former alternative and, apart from morphological considerations, was led to this view by the fact that he had observed aerial stems arising in many instances, as buds on the horizontal branches of *Stigmaria*. Confirmation of this mode of growth is still required, but it is quite conceivable that there may have been a mode of vegetative reproduction in the *Stigmariæ* analogous with that of *Ophioglossum*.[2]

The alternative interpretation of the Stigmarian axes as special morphological entities has received weighty support from Scott and Bower, who consider them comparable to the rhizophores of *Selaginella*, which, as is well known, may either be root-bearers, or under certain circumstances become transformed into leafy shoots. This peculiarity has led Goebel to regard them as special members, somewhat intermediate between stems and roots. But though they might therefore be regarded as of a primitive nature, the rhizophores of the Selaginellaceæ seem such specialized structures that I incline to agree with Bower that, as far as their correspondence with *Selaginella* is concerned, the Stigmarian axes would agree most closely with the basal knot formed on the hypocotyl of *Selaginella spinulosa*. Seeing, however, that the nearest living representative of the Lepidodendraceæ is in all

[2] It is of interest in this connection to note that Potonié has recently put forward the suggestion that many of these vertical outgrowths from the more or less horizontal Stigmarian axes, some of which, as figured and described by Goldenberg, taper off rapidly to a point, without any trace of ramification, may be comparable with the conical "knees" of *Taxodium*, and represent woody pneumatophores so common in the swamp cypress and other swamp-inhabiting trees.

probability *Isoëtes,* which Bower has aptly summarized as like "a partially differentiated *Lepidostrobus* seated upon a Lepidodendroid base," we must inevitably consider the root-bearing base of *Isoëtes* as homologous with the branching axes of *Stigmaria,* whatever their morphological nature may have been, and perhaps we shall be on the safest ground if we consider them both as different expressions of the continued growth of the lower region of the plant, which appears to have been a primary feature in the morphology of both these members of the Lycopodiales.

The somewhat considerable difference in external appearance between the homologous organs of these two plants may be considered bridged over by the somewhat reduced axes of *Stigmariopsis* and by the still more contracted base of the Mesozoic *Pleuromoia,* which, in spite of its very different fructification, we may unhesitatingly compare with *Isoëtes* as far as its root-bearing axis is concerned.

I was inclined at one time to seek an analogy for the Stigmarian axis in that interesting primitive structure, the protocorm of *Phylloglossum,* and of embryo Lycopods; but I now consider that the resemblances are largely superficial, and do not rest upon any satisfactory anatomical correspondence.

One of the features which has caused some divergence of opinion in the past as to the morphology of the Stigmarian axis has been the definite quincuncial arrangement and the apparent exogenous origin of the roots borne on these underground organs. Schimper, indeed, considered these two features so characteristic of foliar organs that he suggested that these so-called "appendices" might possibly be metamorphosed leaves. Not quite satisfied with this view, Renault endeavored to establish the existence of two types of lateral organs on the Stigmarian axis, true roots with a triarch arrangement of wood and root-like leaves of monarch type. Williamson, however, clearly showed that the apparent triarch arrangement was really due to the presence at two angles of the metaxylem of the first tracheids of secondary wood, and reasserted the existence of only one type of appendicular organs, agreeing so closely, both in structure and in their orientation to the axis, on which they were borne, with the roots of *Isoëtes* that it would be impossible to deny the root nature of the Stigmarian "appendices" without applying the same treatment to the roots of *Isoëtes.*

Still, so distinguished a paleobotanist as Solms Laubach, after a careful weighing of all the available evidence, continued to uphold Schimper's view of the foliar nature of these outgrowths, both in his "Paleophytologie" and in his memoir on *Stigmariopsis,* in which he stated that he was in complete agreement with Grand' Eury's conclusion: "Que ces organes sont indistinctement des rhizomes et que les Sigillaires n'avaient pas de racines réelles, ainsi que *Psilotum.*" Indeed, in reviewing the account I gave of the occurrence of a special system of spiral tracheids in the outer cortex of the Stigmarian rootlets, Count Solms drew attention to their similarity to the transfusion tissue of Lepidodendroid leaves, and asserted that we have here a further indication of the former foliar nature of these rootlets. Personally, I still adhere to the belief, expressed at the time, that these peripheral cortical tracheids represent a special development required by a plant with an aquatic monarch root of the *Isoëtes* type and a large development of aerial evaporating surface. The fact that the lateral outgrowths from the Stigmarian axis have been generally considered to be exogenous is not a valid

argument against their root nature, as the same origin is ascribed to the roots of *Phylloglossum* and to those produced on the rhizophores of *Selaginella*. Probably, indeed, as Bower points out in his masterly exposition of the "Origin of a Land Flora," in dealing with the Lycopodiales, "the root in its inception would, like the stem of these plants, be exogenous." According to the "recapitulation theory," indeed, the exogenous formation of the roots in the embryo of certain Lycopods, as well as of the first root of *Isoëtes* and the first root of the Filicales, might be regarded as the retention of a more primitive character in these particular organs. The roots of *Stigmaria*, even if exogenous, might therefore merely represent a more ancestral stage. This difference between the roots of *Isoëtes* and the rootlets of *Stigmaria* may, however, be more apparent than real, for my colleague, Dr. Lang, has drawn my attention to the fact that there appear to be in *Stigmaria* remnants of a small-celled tissue on the outside of what has generally been taken to be the superficial layer of the Stigmarian axis, and a careful investigation of this point inclines me to agree with him that very probably the Stigmarian rootlets were actually formed like those of *Isoëtes*, somewhat below the surface layer, which, after the emergence of the rootlets, became partially disorganized. Should this surmise prove correct when apices of *Stigmaria* showing structure come to light, the last real difference between the rootlets of *Isoëtes* and the rootlets of *Stigmaria* will have disappeared, and the view for which Professor Williamson so strongly contended will be finally established.

While a careful comparison of *Isoëtes* with the extinct Lycopodiaceous plants may be taken to finally settle its systematic position, the Psilotaceæ have been somewhat disturbed by such comparisons. Placed formerly without much hesitation in the phylum Lycopodiales, certain features in their organization, such as the dichotomy of their sporophylls, and the structure of their fructification generally have suggested affinity with that interesting group of extinct plants, the Sphenophyllales. Their actual inclusion in this group by Thomas and by Bower may seem, perhaps, somewhat hazardous, considering the differences existing between the Psilotaceæ and *Sphenophyllum;* and the more cautious attitude of Seward, in setting up a separate group for these forms, seems on the whole more satisfactory than forcing these aberrant relatives of the Lycopods into the somewhat Procrustean bed of Sphenophyllales, which necessitates the minimizing of such important differences as the dichotomous branching of the axis and the alternate arrangement of their leaves, though the latter character allows, it is true, of some bridging over. But, even adopting this more cautious attitude, the study of the Sphenophyllales has been of great help in coming to a clearer understanding of certain morphological peculiarities of the Psilotaceæ, quite apart from the flood of light which this synthetic group of Sphenophyllales has thrown upon the relationship of the Lycopodiales to the Equisetales.

More far-reaching in its bearing on the relationships of existing plants has been the study of those interesting fern-like plants which seem to show in their vegetative organs a structure possessing both fern-like and Cycadian affinities. Full of interest as these so-called Cycadofilices were in their vegetative organization, they were destined to rivet on themselves the attention of all botanists by the discovery of their fructifications. No chapter in the recent history of paleobotany is more thrill-

ing than the discovery, by the patient and thorough researches of Professor Oliver, of the connection between *Lyginodendron* and the well-known paleozoic seed, *Lagenostoma*. With Dr. Scott as sponsor, this new and startling revelation met with ready acceptance, and, thanks to the indefatigable energies of paleobotanists, no fossil fern seemed at one time safe from possible inclusion among the Pteridospermæ.

The infectious enthusiasm with which the discovery of the seed-bearing habit of the Lyginodendreæ and the Medullosæ was greeted carried all before it, and we in England, particularly, have perhaps not looked carefully enough into the foundations upon which rested the theory that these groups form the "missing links" between the ferns and cycads. A criticism against the wholesale acceptance of this view has been put forward by Professor Chodat,[3] of Geneva, that distinguished and versatile botanist, whom we have on several occasions had the pleasure of welcoming in our midst. Couched throughout in friendly and courteous language, and full of admiration for the work of those who were concerned in the establishment of the group of Cycadofilices, now termed Pteridospermæ, Professor Chodat suggests that English paleobotanists have not sufficiently appreciated the work of Bertrand and Corneille[4] on the fibro-vascular system of existing ferns, and have not revised, in the light of the researches of these French investigators, the interpretation given to the arrangement of the primary vascular tissues of *Lyginodendron*. In Chodat's opinion the structure of the primary groups of wood found in the stem and in the double leaf-trace of this plant is not directly comparable with the arrangement found in the petiole of existing Cycads. In the latter the bulk of the metaxylem is centripetal, while we have in addition a varying amount of small-celled centrifugal wood towards the outside of the protoxylem, and though separated from it by a group of parenchymatous cells, the bundle may be conveniently described as mesarch. In *Lyginodendron*, and the same applies to *Heterangium*, the primary bundles of the stem appear at first sight to be mesarch too, but in Chodat's opinion, if I understand him correctly, the metaxylem is exclusively centrifugal in its development, but, widening out and bending inwards again, in form of the Greek letter ω, the two extremities of the metaxylem are united on the inside of the protoxylem, forming an arrangement described by Bertrand and Corneille in the case of several fern petioles under the name of "un divergeant fermé."

Several details of structure, such as the type of pitting of the metaxylem elements and the separation of the protoxylem from the adaxial elements of metaxylem by parenchymatous cells, confirm Chodat in his view that the primary bundles of *Lyginodendron* are not really mesarch, and that the stem of *Lyginodendron* is essentially Filicinean in nature. Chodat cites other characters, such as the presence of sclerized elements in the pith, and the absence of mucilage ducts, in support of his view of the purely filicinean affinities of the Lyginodendreæ. The presence of secondary thickening in *Lyginodendron* he regards not as indicative of Cycadian affinity, but merely as another instance of secondary growth in an extinct Cryptogam, taking up very much the position of

[3] Chodat, R., "Les Ptéropsides des temps paléozoïques," *Archives des Sciences physiques et naturelles*, Genève, Tome XXVI., 1908.

[4] Bertrand, C. E., and Corneille, F., "Etude sur quelques characteristiques de la structure des filicinées actuelles," *Travaux et mémoires de l'Université de Lille*, 1902.

Williamson in his earlier controversy with French botanists with regard to the secondary thickening of *Calamites* and Lepidodendreæ. Chodat is also at variance with Kidston and Miss Benson as to the nature of the microspores borne on the fronds of *Lyginodendron* or *Lyginopteris,* as he prefers to call this plant. He certainly figures some very fern-like sporangia, attached to the fronds of *Lyginodendron,* but any one who has worked with the very fragmentary and somewhat disorganized material contained in our nodules knows how difficult it is to be absolutely certain of structural continuity. Neverthless, a reinvestigation of the whole question of the microsporangia of *Lyginodendron* seems to me clearly called for by the publication of Chodat's figures.

As regards the seed-bearing habit of *Lyginodendron,* Chodat adopts wholeheartedly Oliver's correlation of *Lagenostoma* with the fronds of *Lyginodendron,* but would regard the seed, apparently devoid of endosperm at the time of pollination, as a somewhat specialized macrosporic development, of more complex structure, but analogous in its nature to the seed-like organ exhibited by *Lepidocarpon* in another phylum of the Pteridophyta. "In any case," he concludes, "the origin and the biology of this kind of seed must have been very different from those of the seeds of the Gymnosperms."

This contention, based mainly on the tardy development of the endosperm in *Lagenostoma,* is the least weighty part of Chodat's criticism, for it has never been asserted that the seeds were identical with those of existing Cycads. We know that the seed-habit was adopted by various groups of Vascular Cryptogams, and it is revealed in fossil plants in various stages of evolution, so that it may be readily presented to us at a special stage of its evolution in *Lyginodendron.* Moreover, we must remember that in so highly organized a Gymnosperm as *Pinus,* the macrospore itself is not fully developed at the time of pollination. Though not suggesting this as a primitive feature in the case of the pine, we can well imagine how, by a gradual process of "anticipation," the prothallus might become established before pollination in any group of primitive seed-bearing plants. There are other more specialized rather than primitive features in the complex structure of *Lagenostoma* which might with much more reason be invoked, to show that the seed of *Lyginodendron* does not form a step in the series of forms leading to the Cycadian ovule.

But leaving this point out of consideration, Chodat brings forward some strong reasons for his conclusions that the Lyginodendreæ were plants possessing stems of purely fern-like structure, increasing in thickness by means of a cambium, that their foliage was of filicinean structure, but provided with two kinds of sporangia, microsporangia similar to those of Leptosporangiate ferns, and macrosporangia of specialized type, containing a single macrospore. This group, therefore, Chodat regards as a highly specialized group of ferns, which, he considers, shows no particular connection with the Cycads, and which may have formed the end in a series of highly differentiated members of the Filicineæ.

Of the Medullosæ, on the other hand, Chodat takes a very different view. Both in the structure of their primary and secondary growth, as well as in their polystely, he sees close affinity of these forms to the Cycads, borne out by smaller secondary features, such as the presence of mucilage ducts, and the simple form of pollen-chamber. Chodat considers the agreement of the Medullosæ with the Cycadaceæ to be so

close that he regards them as Protocycadeæ, the fern-like habit being restricted to the position of the sporangia on the vegetative fronds. *Medullosa*, therefore, would be only one link in the chain connecting the Cycads with the Filicales, and a link very near the Cycadian end of that chain. Other forms more closely connected with the Filicinean phylum are still to be sought.

In bringing Professor Chodat's views to your notice, I do not wish to urge their acceptance, but his criticism seems to me sufficiently weighty to demand a careful reconsideration of the structure and affinities of the Lyginodendreæ, which, whatever may be their ultimate position in our scheme of classification, will continue in the future, as they have done in the past, to command the attention of all botanists interested in the evolution of plant life.

If the whole-heartedness with which we in England received the theory of the Cycadian affinity of *Lyginodendron* has laid us open to friendly criticism, I am afraid some of us may be accused of exceeding the speed-limit in our rapid acceptance of the Cycadoidean ancestry of the Angiosperms. Ever since Wieland put forth the suggestion in his elaborate monograph of the "American Fossil Cycads" that "further reduction and specialization of parts in some such generalized type, like the bisporangiate strobilus of *Cycadoidea*, could result in a bisexual angiospermous flower," speculation as to the steps by which the evolution might have been brought about has been rife, and Hallier in Germany and Arber and Parkin in England have put forward definite schemes giving probable lines of descent. Arber and Parkin in their criticism and detailed suggestions connect phylogenetically with the Bennettitales, the Ranales, as primitive Angiosperms, and displace from this position the Amentales and Piperales, which were regarded by Engler as probably more closely related to the Proangiosperms. Of course, the resemblance between the amphisporangiate, or, as I should prefer to call it, the heterosporangiate "strobilus" of *Cycadoidea*, and the flower, say, of *Magnolia* is very striking, and the knowledge we have gained of the structure and organization of the Bennettitales certainly invites the belief in a possible descent of the Angiosperms from this branch of the great Cycadian plexus; but the ease with which the flower of the Ranales can in some respects be fitted on to the "flower" of *Cycadoidea* raises suspicion. Critics of the Arber-Parkin hypothesis may possibly incline to the view that "truth is often stranger than fiction," and that the real descent of the Angiosperms may have been much less direct than that put forward in these recent hypotheses. The particular view of the morphology of the intraseminal scales and seed pedicles adopted by Arber and Parkin is, as they admit, not the only interpretation that can be put upon these structures, and the views on this point will probably remain as various as are those of the female cone of *Pinus*. Even if we regard the ovulate portion of the Cycadoidean "flower" as a gynecium, and not as an inflorescence, we are bound to admit, as do Arber and Parkin, that it is highly modified from the pro-anthostrobilus type with a series of carpels bearing marginal ovules. *Cycadoidea* was evidently a highly specialized form, and may well have been the last stage in a series of extinct plants.

Arber's very sharp separation of mono- and amphisporangiate Pteridosperms does not seem to me quite justified. Amphisporangiate forms may have been preserved, or may have arisen anew in various groups of Pteridosperms or in their descendants. Heterospory, we know, orig-

inated independently in at least three of the great phyla of vascular Cryptogams, and originally, no doubt, the same strobilus contained both macro- and microsporangia, as was the case in *Calamostachys Casheana,* in the strobili of most Lepidodendraceæ, and as is still the case in the strobili of *Selaginella* and in *Isoëtes.* Even in the existing heterosporous Filicineæ, micro- and macrospores are found on the same leaf and on the same sorus; and though in the higher Cryptogamia and the lower Phanerogamia there may have been a tendency to an iso-sporangiate condition, yet, as the two kinds of spores are obviously homologous in origin, nothing is more natural than an occasional reversion to a heterosporangiate fructification. Thus in the group of Gymnosperms we have many instances of the occurrence of so-called androgynous cones. In 1891, at the meeting of the British Association in Leeds, I described such amphisporangiate cones which occurred regularly on a *Pinus Thunbergii* in the Royal Gardens of Kew, and only this spring I was able to gather several hermaphrodite cones of *Larix Europea.* They have, of course, been observed and described by many authors for a variety of Gymnosperms. What more likely than that many extinct Gymnosperms may have developed heterosporangiate fructifications? It is not necessary, therefore, to fix on one group of ancestors for the origin of all existing Angiosperms. Indeed, the great variety of forms, both of vegetative and reproductive organs, which we meet with in the Angiosperms, not only to-day, but even in the Cretaceous period, in which they first made their appearance, warrants, I think, the belief in a polyphyletic origin of this highest order of plants. It is no doubt true, as Wieland points out "that the plexus to which *Cycadoidea* belonged, as is the case in every highly organized plant type, presented members of infinite variety," and, indeed, so far as the vegetative organization goes, we know already, through the labors of Nathorst, of such a remarkable form as *Wielandiella angustifolia,* while Wieland has shown us a further type in his Mexican *Williamsonia.* Nevertheless, these diverse forms all agree in the structure of their gynecium, the particular organ which is not so easy to bring into line with that of the Angiosperms.

I am quite alive to, though somewhat sceptical of, the possibility of a direct descent of the Ranales from the Cycadoideæ, but my hesitation in accepting Arber and Parkin's view of the ancestry of the Angiosperms is enhanced by the consideration that it seems almost more difficult to derive some of the apparently primitive Angiosperms from the Ranales, than the latter from Cycadoidea. Indeed, this common origin of Angiosperms from the Ranalian plexus will, I feel sure, prove the stumbling-block to any general acceptance of the Arber-Parkin theory. It is easy enough to assume that all Angiosperms with the unisexual flowers have been derived by degeneration or specialization from forms with hermaphrodite flowers of the primitive Ranalian type, but unfortunately some of these degenerate forms possess certain characters which appear to me to be undoubtedly primitive.

It is difficult for those who accept Bower's view of the gradual sterilization of sporogenous tissue not to regard the many-celled archesporium in the ovules of *Casuarina* and of the *Amentales* as a primitive character, and though, as Coulter and Chamberlain point out, this feature is manifested by several members of the Ranunculaceæ and Rosaceæ, as well as by a few isolated Gamopetalæ, its very widespread occurrence in the Amentales seems

to indicate its more general retention in this group of plants, and does not agree readily with the theory that these unisexual orders are highly specialized plants, with much-reduced flowers. The possession of a multicellular archesporium is, however, not the only primitive character exhibited by some of the unisexual orders of the Archichlamydeæ. Miss Kershaw[5] has shown, in her investigation of the structure and development of the ovule of *Myrica*, that in this genus, which possesses a single erect ovule, the integument is entirely free from the nucellus, and is provided with well-developed vascular bundles, in both of which features it resembles very closely the paleozoic seed *Trigonocarpus*. The same features were shown, moreover, by Dr. Benson[6] and Miss Welsford to occur in the ovules of *Juglans regia* and in a few allied genera, such as *Morus* and *Urtica*. Also in a large number of Amentales with anatropous ovules (*Quercus, Corylus, Castanea*, etc.), Miss Kershaw has demonstrated the occurrence of a well-developed integumentary vascular supply. No doubt a further search may reveal the occurrence of this feature in some other dicotyledonous ovules, but in the meantime it seems difficult to believe that such a primitive vascular system, which the Amentales share with the older Gymnosperms, would have been retained in the catkin-bearing group, if it had undergone far-reaching floral differentiation, while it had disappeared from the plants which in other respects remained primitive. It would be still more difficult to imagine that it had arisen in the Amentales subsequently to their specialization.

There are other structural characters and general morphological considerations, which I have not time to deal with, which underlie the belief in the primitiveness of the Amentales and some allied cohorts, and I trust they will be set forth in detail by a better systematist than I can claim to be. My object in bringing the matter forward at all is to point out some of the difficulties which prevent me from accepting a monophyletic origin of the Dicotyledons through the Ranalian plexus.

One of these difficulties lies in the relationship of the Gnetales to the Dicotyledons. Arber and Parkin have recently made the attempt to gain a clearer insight into the affinities of this somewhat puzzling group by applying to it the "strobilus theory[7] of Angiospermous descent." The peculiar structure of the flowers of *Welwitschia* lends itself particularly well to a comparison with those of *Cycadoidea*, and a good case can no doubt be made out for a hemiangiospermous ancestry of this member of the Gnetaceæ, and by reduction the other members, in many respects simpler, might be derived from a similar ancestor, though probably, as far as *Ephedra* and *Gnetum* are concerned, an equally good, if not better, comparison might be made with *Cordaites*. But even supposing we admit the possibility of a derivation of the Gnetales from an amphisporangiate Pteridosperm, I think the Amentales merit quite as much as the Gnetales to be considered as having taken their origin separately from the Hemiangiospermæ, and not from the Ranalian plexus. I find this view has been put forward also by Lignier[8] in his attempt to reconstruct the phylogenetic history of the Angiosperms, and I feel strongly that

[5] *Annals of Botany*, Vol. XXIII., 1909.

[6] *Ibid.*

[7] Arber, E. A. N., and Parkin, J., "Studies on the Evolution of Angiosperms," "The Relationship of the Angiosperms to the Gnetales," *Annals of Botany*, Vol. XXII., 1908.

[8] Lignier, O., "Essai sur l'Evolution morphologique du Règne vegetal," *Bull. de la Soc. Linnéenne de Normandie*, 6 sér., 3 vol., 1909, reimprimé février, 1911.

such a polyphyletic descent, whether from the more specialized anthrostrobilate Pteridospermæ or from several groups of a more primitive Cycado-Cordaitean plexus, is more in accordance with the early differentiation of the Cretaceous Angiosperms, and with the essential differences existing now in the orders grouped together as Archichlamydeæ.

Attempts at reconstructing the phylogeny of the Angiosperms are bound to be at the present time largely speculative, but we may possibly be on the threshold of the discovery of more certain records of the past history of the higher Spermaphyta, since Dr. Marie Stopes has commenced to publish her investigations of the cretaceous fossil plants collected in Japan, and Professor Jeffrey has been fortunate enough to discover cretaceous plant-remains showing structure in America. The former have already provided us with details of an interesting Angiospermic flower, and if the latter have so far only yielded Gymnosperms, we may at all events learn something of the primitive forms of these plants, the origin of which is still as problematical as is that of the Angiosperms.

I trust that the criticisms I have made of the theory put forward by Messrs. Arber and Parkin will not be taken as a want of appreciation on my part of the service they have done in formulating a working hypothesis, but merely as an expression of my desire to walk circumspectly in the very alluring paths by which they have sought to explore the primeval forest, and not to emulate those rapid but hazardous flights which have become so fashionable of late.

While the description of new and often intermediate forms of vegetation has aroused such wide-spread and general interest in paleobotany, other and more special aspects of the subject have not been without their devotees, and have proved of considerable importance. Morphological anatomy has gained many new points of view, and our knowledge of the evolution of the stele owes much to a careful comparison of recent and fossil forms, even when these investigations have produced conflicting interpretations and divergent views.

Another promising line of paleobotanical research lies in the direction of investigations of the plant tissues from the physiological and biological points of view. Happily, the vegetable cell-wall is of much greater toughness than that of animal cells, and in consequence the petrified plant-remains found in the calcareous nodules are often so excellently preserved that we can not only study the lignified and corky tissues, but also the more delicate parenchymatous cells. Even root-tips, endosperm and germinating fern-spores are often so little altered by fossilization that their cells can be as easily studied as if the sections had been cut from fresh material. It is this excellence of preservation which has enabled us to gain so complete a knowledge of the anatomy of paleozoic plants, and since the detailed structure of plant organs is often an index of the physical conditions under which the plants grew, we are able to form some opinion as to the habitat of the coal-measure plants. Though a beginning has already been made in this direction by various authors, we have as yet only touched the fringe of the subject, and, as Scott points out in the concluding paragraph of his admirable "Studies," the biology and ecology of fossil plants offer a wide and promising field of research. Such studies are all the more promising, as we now have material from such widely separated localities as the Lancashire coalfield, Westphalia, Moravia and the Donetz Basin in Russia.

Now that it has been definitely shown by

Stopes and Watson that the remains of plants are sometimes continuous through adjacent coal-balls, we may safely accept their conclusion that these calcareous concretions were in the main formed *in situ,* and that the plant-remains they contain represent samples of the vegetable débris of which the coal-seam consists. We have in these petrifactions, therefore, an epitome, more or less fragmentary, of the vegetation existing in paleozoic times on the area occupied by the coal seam, and the Stigmarian roots in the underclay, as well as other considerations, lead us to believe that the seam more frequently represents the remains of the coal-measure forest carbonized *in situ.* While this seems to be the more usual formation of coal-seams, it is obvious from the microscopic investigations of coal made by Bertrand, and as has recently been so clearly set forth by Arber in his "Manual on the Natural History of Coal," that in the case of bogheads and cannels the seam represents metamorphosed sapropelic deposits of lacustrine origin. In other cases, again, considerations of the nature of the coal and the adjacent rocks may incline us to the belief that some, at any rate, of the deposits of coal may be due to material drifted into large lake-basins by river agency.

Broadly speaking, however, and particularly when dealing with the seams from which most of our petrified plant-remains have been collected, we may consider the coal as the accumulated material of paleozoic forests metamorphosed *in situ.* What, then, were the physical and climatic conditions of these primeval forests? The prevalence of wide air-spaces in the cortical tissues of young Calamitean roots, as indeed their earlier name Myriophylloides indicates, leads us to believe that, as in the case of many of their existing relatives, they were rooted under water or in water-logged soil. We gather the same from the structure of *Stigmaria,* while the narrow xerophytic character of the leaves at any rate of the tree-like *Calamites* and *Lepidodendra* closely resembles the modifications met with in our marsh plants. It has been suggested by several authors that the xerophytic character of the foliage of many of our coal-measure plants may be due to the fact that they inhabited a salt marsh. A closer examination of the foliage, however, of such plants as *Lepidodendron* and *Sigillaria* does not reveal the characteristic succulency associated with the foliage of most Halophytes, and in view of the absence of such water-storing parenchyma, the well-developed transfusion-cells of the Lepidodendreæ can only be taken to be a xerophytic modification such as is met with in recent Conifers.

The specialization of the tissues indeed is only such as is quite in keeping with the xerophytic nature of marsh plants. Moreover, the particular group of Equisetales are quite typical of fresh water, and we should expect that if their ancestors had been Halophytes, some, at any rate, at the present day would have retained this mode of life. Nor have we at the present time any halophytic Lycopodiales, while *Isoëtes,* the nearest relative to the *Lepidodendra,* is an aquatic or sub-aquatic form associated with fresh water.

Among the Filicales, *Acrostichum aureum* seems to be the only halophytic form, inhabiting as it does the swamps of the Ceylon littoral,[9] and though, as Miss Thomas has pointed out, its root structure is in close agreement with that of many paleozoic plants, its frond shows considerable deviation from that of *Lyginodendron* or *Medullosa,* both of which plants, as

[9] Tansley, A. G., and Fritsch, "The Flora of the Ceylon Littoral," *New Phytologist,* Vol. IV., 1905.

Pteridosperms, are on a higher plane of evolution, and might therefore be expected to show a more highly differentiated type of leaf. But on the contrary these coal-measure plants show a more typically Filicinean character, both as regards the finely dissected lamina and also in the more delicate texture of the foliage compared with the specialized organization of the frond of *Acrostichum aureum*, described by Miss Thomas.

Nor is it necessary to call to aid the salinity of the marsh to explain the excellent preservation of the tissues of the plant-remains in the so-called coal-balls, in view of the well-known power of humic compounds to retard the decay of vegetable tissues. In addition to these arguments, I might draw attention to the presence of certain fungi among the petrified débris, as more likely to be found in fresh water than in marine conditions. *Peronosporites*, so common in the decaying Lepidodendroid wood, and the *Urophlyctis*-like parasite of Stigmarian rootlets, seem to me to support the fresh-water nature of the swamp; just as the occurrence of the mycorhiza, described by Osborn, in the roots of *Cordaites* seems to indicate the presence of a peaty substratum for the growth of that plant. Potonié also refers to the occasional occurrence of Myriapoda and fresh-water shells as indicative of the fresh-water origin of at least many of the coal deposits, and a common feature of the petrified remains of coal-measure plants is the occurrence of the excrements of some wood-boring larvæ in the passages tunneled by these paleozoic organisms through the wood of various stems.

A strong argument in favor of the brackish nature of these swamps would be supplied by the definite identification of *Traquairia* or *Sporocarpon* as Radiolaria, though we must remember that certain marine Cœlenterata find their way up into the Norfolk Broads, the fresh-water Medusæ are by no means unknown in different parts of the tropics. Of course, if the coal-measure swamps were estuarine or originated in fresh-water lagoons near the sea, they may have been liable from time to time to invasions of salt water, sufficient to account for the presence of occasional marine animals, but without constituting a halophytic plant association.

Potonié, who has made so close a study of the formation of coal, and who supports the theory of its fresh-water origin, considered for a long time the comparison between the coal-measure swamp and the cypress swamps of North America, as the nearest but at the same time a somewhat remote analogy, more particularly as he believed that the nature of the coal-measure vegetation required a tropical and also a moister climate than obtains in the southern states of North America. Though, in view of the great development of Pteridophytic vegetation in countries like New Zealand, I think Potonié possibly exaggerates the temperature factor, he is probably right in assuming a fairly warm climate for the coal-measure forest. The difficulty, so far, has been to account for the great thickness of humic or peaty deposits which must have accumulated for the formation of our coal-seams, in view of the fact that extensive peat formation is generally associated with a low temperature. In the tropics, peat may be deposited at high altitudes, where there is low temperature and high rainfall, but it is generally supposed that the rate of decomposition of vegetable remains is so active that lowland peat-formation was out of the question. Dr. Koorders, however, has observed a peat-producing forest in the extensive plain on the east side of Sumatra, about a hundred miles from the coast. This swamp-forest

has been recently reexplored at the instance of Professor Potonié, and he finds it to agree closely with the vegetative peculiarities which he considers must have been presented by the vegetation of the coal-measure forest. A typical "Sumpflachmoor," this highly interesting tropical swamp has produced a deposit of peat amounting in some places to thirty feet in thickness. The peat itself consists mainly of the remains of the Angiospermic vegetation of which the forest is made up, including pollen-grains and occasional fungal filaments; the preservative power, which has enabled this accumulation of débris to take place, being due to the peaty water which is seen above the roots of the bulk of the vegetation. The latter consists mainly of dicotyledonous trees belonging to various natural orders, and they mostly show such special adaptations as breathing roots (pneumatophores) and often buttress roots. With the exception of a tree-fern, Pteridophyta, Liverworts and Mosses, and, indeed, all herbaceous vegetation, are poorly represented in this swamp, though high up in the branches of the trees there are a fair number of epiphytes, and at the edge of the swamp-forest lianes, belonging particularly to the palms, play an important part in the vegetation. The water, partly on account of its peaty nature, partly owing to the intense shade, is almost devoid of algæ, and none of these organisms were found in the peat itself. The interesting account given by Potonié of this tropical peat-formation is very suggestive when certain features, as, for example, the absence or relative paucity of certain of the lower groups of plants, such as algæ and Bryophyta, in the peat, are compared with the plant-remains in some of our coal-seams. Replacing the now dominant Angiosperms by their Pteridophytic representatives in paleozoic times, we have a very close parallel in the two formations.

Another interesting question arises when we consider the great variety of types of vegetation met with among the plant-remains of the coal-seams. For in addition to the limnophilous *Calamites* and Lepidodendraceæ mentioned above, the coal-balls abound with the remains of representatives of the Filicales, the Pteridospermæ and the Cordaitaceæ. Were these also members of this swamp vegetation, or have their remains been carried by wind or water from surrounding areas? With regard to some plant-remains, namely, those found exclusively in the roof nodules, the latter was undoubtedly the case; for we have ample evidence, both in their preservation and their mode of occurrence, that they have drifted into the region of the coal-measure swamp after its submergence below the sea. This would apply to such plants as *Tubicaulis Sutcliffii* (Stopes), *Sutcliffa insignis* (Scott), *Cycadoxylon robustum* and *Poroxylon Sutcliffii* and other forms, the remains of which have so far not been observed in the coal-seam itself. These plants represent a vegetation of non-aquatic type, and may be taken to have grown on the land areas surrounding the paleozoic swamps. But, on the other hand, we have remains of many non-aquatic plants in the coal-seam itself, closely associated with fragments of typical marsh-plants. How can their juxtaposition be explained?

The advance of our knowledge of ecology points, I think, to a solution of this difficulty. No feature of this fascinating study, which has of late gained so prominent a place in botanical investigation, is more interesting than to trace out the succession of plant associations within the same area, noting the ever-changing conditions which the development of each as-

sociation brings about. If we follow with Schroeter the gradual development of a lacustrine vegetation from the reed-swamp through the marsh (or Flachmoor) to a peat-moor (Hochmoor), we see how one plant association makes place in its turn for another. May not the mixture of various types of vegetation which we meet with in the petrifactions of our coal-seam represent the transition from the open Calamitean or Lepidodendroid swamp to a fen or marsh with plentiful peat-formation, due to the gradual filling up of the stagnant water with plant-remains? Thus in places, at any rate, a transition from aquatic to more terrestrial types of vegetation would take place, while the tree-like forms rooted in the deeper water would continue to flourish. The coal-measure swamp in this stage would differ from the tropical swamp of Kooders by a more abundant undergrowth of herbaceous and climbing plants, rooted in damp humus and passing off gradually into drier peat. Such an undergrowth of Cryptogamic types, mainly Filicinean or Pteridospermic, would have admirable conditions for luxuriant development, apart from the provision of a suitable substratum for its roots, owing to the narrow xerophytic nature of the foliage on the canopy of the trees under which it grew.

Here, too, we see the explanation of the striking difference between the microphyllous and arborescent *Calamites* and Lepidodendraceæ, and the large ombrophile foliage of the Filicineæ and Pteridosperms, which spread out their shade-leaves under the cover of marsh xerophytes, in exactly the same way as Professor Yapp has so admirably depicted for recent plants in his account of the "Stratification in the Vegetation of a Marsh."

The development of a mesophytic vegetation in the shelter of the marsh xerophytes makes it unnecessary to postulate an obscuration of the intense sunlight by vapors, as was done by Unger and Saporta for the Carboniferous period. The assumption of a variety of conditions of plant life within the same area helps materially to clear up the difficulties presented by the somewhat incongruous occurrences met with in the petrified plant-remains. The presence of fragments of *Cordaites,* mixed with those of *Calamites* and *Lepidodendra,* in the coal-balls can not always be explained either by a drift theory, or by conceiving the fragments to be wind-borne; but, given an area of retrogressive peat above the ordinary water-level, and even so xerophytic a plant as *Cordaites* might well establish itself there, its mycorhiza-containing roots being well adapted for growth in drier peat. The curious occurrence of more or less concentric rings in the secondary wood of the stem and roots of *Cordaites* may represent a response probably not to annual variations of climate, but to abnormal periods of drought, which would affect the upper-peat layers, but not the water-logged soil in which were rooted the *Calamites* and *Lepidodendra.*

If, as I suspect, we had in the peat deposit of the coal-seam a succession of associations, we ought to find its growth and history recorded by the sequence of the plant-remains, very much as Mr. Lewis has discovered with such signal success in our Scottish peat-bogs. That some differences occur in the plant-remains building up a seam can be noted by a microscopic examination of the coal itself, in which, as Mr. Lomax tells me, the spores of *Lepidodendra* occur in definite bands. But no systematic attempt has as yet been made to investigate from this point of view the seams charged with petrified plant débris. Before the Shore pit, which was reopened last

summer through the renewed generosity of Mr. Sutcliffe, was finally closed down, I obtained two series of nodules, ranging from the floor to the roof of the seam, and have had these cut for detailed examination. I should not, however, like to make any generalizations from these isolated series, but intend, during the coming winter, to investigate in the same manner further series taken from large blocks of nodules, which have been removed bodily so as to retain the position they occupied in the seam. Though at present the data are only fragmentary, there seems to be some indication that the plant-remains are not without some relation to their position in the seam. Of course, Stigmarian rootlets are ubiquitous, and in the nodules of the lower part of the seam predominant, but other plant-remains appear to be more frequently found at one level of the seam than another. The problem, however, is very involved, and it has become apparent that it is as important to study the fine débris in which the larger fragments are embedded as the distribution of these latter. Moreover, attention must be paid to the stage of decomposition presented by the particles forming the matrix of the nodule, as this varies in the lower and upper parts of a seam, very much as in a peat-bed we can distinguish the lighter-colored fibrous peat from the darker layers at the base of a peat-cutting. Mr. Lomax, who has so unique an experience of these coal-balls, informs me that he can tell whether a nodule is from the top or bottom of the seam by the lighter or darker color of the matrix. The importance of applying the methods which have been so successful in elucidating the history of modern peat-deposits to the investigation of the coal-seam will be clearly appreciated both by paleobotanists and ecologists, and this particular problem offers a striking illustration of the interdependence of various branches of botanical investigation. It is fortunate indeed that the two fields of work, paleobotany and plant ecology, though they have been subjected to fairly intensive cultivation, have not become exclusively the domain of specialists. The strength and progress of modern botany have been due to the close collaboration of workers engaged in different branches of botanical science, and the fact that British ecologists have combined to attack a series of the problems from very diverse points of view leads one to hope that, with a continuance of that intimate cooperation which has characterized their work so far, and with the added stimulus of the friendly visit of our distinguished colleagues from abroad, considerable progress may be expected in the future in this branch of botanical study. Privileged as I have been to assist at the deliberations of the British ecologists, without as yet having taken any active part in their work, I feel myself at liberty to point with appreciation to the excellent beginning they have made of a botanical survey of Great Britain and Ireland, as well as to the more detailed investigations of special associations and formations, such as the woodlands, the moorlands, the fens, the broads, salt marshes and shingle beaches. I am glad to think that our foreign visitors have been able to see these interesting types of vegetation under the guidance of those who have made a special study of these subjects.

The importance to ecologists of an up-to-date critical flora was dwelt upon by my predecessor in this presidential chair, and this obvious need may be regarded as a further illustration of the interrelationship of the various aspects of botanical science. Though it has been obvious to all that the swing of the pendulum has been for a long time away from pure systematic

botany, I am convinced that the great development of plant ecology, of which we have many indications, will not merely lessen the momentum of the swinging pendulum, but will draw the latter back towards a renewed and critical study of the British flora. That a revival of interest in systematic botany will come through the labors of those who are engaged in survey work and other forms of ecological study, is foreshadowed by the fact that Dr. Moss has undertaken to edit a "New British Flora," which will, I believe, largely fulfill the objects put forward by Professor Trail in his presidential address. I trust, however, that in addition to the ecologists, those botanists who are interested in genetics will contribute their share towards the completion of our knowledge of critical species, varieties and hybrids, all of which offer such intricate problems alike to the systematist and to the student of genetics.

De Vries prefaced his lectures on "Species and Varieties, their Origin by Mutation," by the pregnant sentence: "The origin of species is an object of experimental investigation," and this is equally true of the study of the real and presumptive hybrids of our British flora, which may be investigated either synthetically or, when fertile, also analytically, as in some cases their offspring show striking Mendelian segregation. Some good work has already been accomplished in this direction, but more remains to be done, and we have here an important and useful sphere of work for the energies of many skilled plant-breeders.

I would, therefore, like to plead for intimate collaboration between all botanists, hopeful that, as progress in the past has come through the labors of men of wide sympathies, so in the future, when studies are bound to become more specialized, there will be no narrowing of interests, but that the various problems which have to be solved will be attacked from all points of view, the morphological, the physiological, the ecological and the systematic. Thus by united efforts and close cooperation of botanists of all schools and of all countries we shall gain the power to surmount the difficulties with which our science is still confronted.

F. E. Weiss

WORK AT THE MARINE BIOLOGICAL STATION OF SAN DIEGO DURING THE SUMMER OF 1911

Because a majority of the station's staff are still holding college positions and hence can be at La Jolla only during vacation time, the summer months are the most active of the year. This disadvantage must continue until the income is sufficient to maintain operations at sea and a considerable part of the work in the laboratory throughout the year.

On the biological side the most important event of the year is the final issuance of Mr. E. L. Michael's "Classification and Vertical Distribution of the Chætognatha of the San Diego Region" (Univ. Calif. Public. Zoology, Vol. 8, No. 3, 165 pp.). In this the author not only records all the species so far taken in these waters and subjects the description and taxonomy of the group to a critical examination, but deals quantitatively with the large amount of data collected during the five years from 1904 to 1909.

The aim of the ecological aspect of the investigation was to ascertain the facts concerning the distribution, seasonal and vertical, of the organisms, and to see how far these are correlatable with and hence dependent upon the environmental factors of light, and of temperature and density of the water. Efforts were limited to these three environmental factors simply because the scope of the station's work up to this time has not made it possible to extend the hydrographic observations beyond these.

While it is impossible to summarize here

the results reached, a few, particularly significant, may be indicated.

Comparison of the distribution of the ten species occurring in the area "shows conclusively that the manner of distribution is correlated with the organization of the species. In other words, the distinctive manner in which each species is distributed is as much a *specific character* as is its structure." For example, each species appears to have its own depth of maximum abundance, spoken of by Mr. Michael as its "center of migration." This is certainly true of the more abundant species, and seems to be true of those which do not occur in sufficient numbers to render the results so far obtained entirely conclusive. From 15 to 20 fathoms is found to be the center of migration of *Sagitta bipunctata,* the most abundant species, while between 100 and 200 fathoms is the center for *S. serratodentata.* These results strengthen the growing idea of species characters in the habits of organisms, and so are of considerable general importance.

The correlations of distribution with temperature and also with density, which are distinctly indicated if not fully proved, are, in several ways, rather unexpected and surprising, particularly as regards density correlations. If these results are borne out by further observations they will still further emphasize the complexity of *orderly interrelations* that prevail between organic beings and their surroundings.

Professor Kofoid has this summer published numbers IV. and V. of "Dinoflagellata of the San Diego Region" (Univ. of Calif. Publ. Zoology, Vol. 8, Nos. 4 to 6, 106 pp.). By far the more extensive of these is No. 4 dealing exclusively with *Gonyaulax.*

It is hardly to be supposed that all the species occurring in the area have now been recorded. Nevertheless, the author has felt his experience with the group to be extensive enough to justify him in giving the genus a thorough overhauling. This he has done largely on the basis of local material but also partly on that of collections from other parts of the world. Of the 25 species recognized as belonging to the revised genus 14 are recorded from the San Diego region, four of which are new. One species formerly included in Gonyaulax, *G. jollifer,* has been made the basis of a new genus, *Spiraulax.*

The author is, warrantably it seems, particularly impressed with "the all-pervading character of specific differences in both morphological and physiological details" among these organisms.

Special attention is given to *G. polyedra,* the species chiefly responsible for the "red-water" and wonderful display of phosphorescence that occurs in this region at times.

It would appear that Professor Kofoid is well ready for the next steps—study of the distribution, reactions, propagation, etc., of these organisms.

Dr. C. O. Esterly, who up to this time has been able to do little more than describe and record the great number of species of copepods of the region, has now taken seriously in hand the problems of seasonal and vertical distribution in this group. The summer has enabled him to advance the task well beyond the points indicated by his two recent papers, "The Vertical Distribution of *Eucalanus elongatus* in the San Diego Region during 1909" (Univ. Calif. Public Zoology, Vol. 8, No. 1) and "Diurnal Migration of *Calanus finmarchicus* in the San Diego Region during 1909" (*Intern. Rev. d. ges. Hydrobiol. u. Hydrogr.,* Bd. 4).

Treatment of the data relative to the distribution of some of the species of *Salpa* is now under way by Mr. Michael and the Director and it is hoped that a report on this group may be ready some time during the winter.

In order to extend the migrational studies, on the one hand, to animals better endowed than the groups already treated, with the powers of vision and locomotion, and, on the other hand, to those less well provided with organs of light perception and locomotion, it is proposed to take up, as soon as possible, the schizopoda and the ctenophora.

Now that considerable headway has been made in investigating the distribution and

movements of pelagic animals as they occur in nature, the desirability of subjecting the same groups to laboratory experimentation is more obvious than ever. This aspect of the investigations can not be entered upon advantageously until the salt water supply system for the laboratory and landing facilities for boats shall have been completed. A large expenditure of money will be needed for these extensions, but vigorous efforts will be made to accomplish the work within the next twelve or eighteen months.

Although the hydrographic investigations by the station are held to be primarily in the interest of biological problems, at the present moment some of the results being reached are of themselves so important as to make them closely rival in interest the biological work itself. Dr. G. F. McEwen, who has charge of this side of the researches, has devoted himself almost entirely, during this and the preceding summer, to testing V. W. Ekman's theory of oceanic circulation so far as it applies to the phenomena of upwelling waters along the continental margins of the great oceans. The study has gone far enough to make it clear that radical modification will have to be made of the widely held supposition that the low summer temperature of the sea along the west coast of North America south of Point Conception, is due to a "California branch of the Japan Current." Just how far this modification will have to go can not be determined without extending the temperature observations much farther to sea, and as far south as the extremity of Lower California at least.

A particularly interesting point in connection with Ekman's theory is the relation which it assumes to exist between water temerature at the surface, as well as at different depths, and bottom topography. In a region like that in which we are operating, where there is a large area of continental shelf presenting numerous islands, banks, deep valleys and channels, that is, of varied bottom configuration, this element is specially important, not only in itself, but probably in its bearing on the distribution of pelagic organisms.

Mr. W. C. Burbridge, of Stanford University, who for the last two years has had in hand most of the laboratory work on water samples, has been more successful this summer than before in manipulating the rather complicated Fox apparatus for determining the gaseous content of the water. A large number of determinations were made, but the data have not yet been worked up, so it is impossible to say how valuable they are. The impression gained is that the method, even with its obvious and necessary limitations, will give results that can be safely and significantly used as one more factor in studying the environmental conditions of pelagic organisms.

During the summer the Station has entered into relations with the California State Game and Fish Commission for the economic study of the "spiny lobster" (*Palinurus interruptus*); and with the Bureau of Soils of the United States Department of Agriculture for estimating the quantity of "kelp" (*Macrocystis pirifera* and *Nereocystis Luetkeana*) on the coast south of Point Conception for such industrial purposes as these plants may be turned to.

Dr. B. M. Allen, of the University of Wisconsin, has charge of the former work, and his searching inquiry into the lobster fishery as it is now prosecuted not only on the California but also on the Mexican coast, has already brought to light various facts which, if followed up, should aid materially in formulating legislation for the preservation of this industry.

The work for the Bureau of Soils is in the hands of Mr. W. C. Crandall, now acting in the three-fold capacity of teacher of biology in the State Normal School at San Diego, secretary of the Marine Biological Association, and captain of the station's scientific boat, the *Alexander Agassiz*.

These industrial undertakings are, at present, aside from the main aims of the station. This, however, is in no wise due to lack of sympathy on the part of the chief patrons and the officials of the Biological Association with such undertakings, but entirely to the

circumstance that, under the present limitations of income, it seems wisest to make research the primary object. Consequently, whenever, as in these cases, it happens that equipment and experience can be made to serve industrial ends without considerable interference with research, the management is more than glad to thus extend the station's usefulness.

Worthy of record is the solution that has been reached, so far as the work itself is concerned, of the problem of manning the *Agassiz* satisfactorily to the purposes for which she exists. Considerable difficulty has been experienced heretofore in finding a man who should be at the same time seaman and scientist enough to get the best results from the operations at sea. Mr. Crandall, when not occupied in the school room, had spent much of his time on the boat looking after the scientific work. He finally decided that with a little more preparation he could manage the whole business, boat and all, more satisfactorily than it was being done. Consequently, he successfully took the examination for a license as master of a boat of the class to which the *Agassiz* belongs. The result is that the boat, under his command, is being operated more efficiently, smoothly and economically in proportion to the work done than ever before. The one serious difficulty is, of course, that school duties make it impossible for him to go with the boat at times when it is very important for her to be at sea.

Although in a number of respects conditions at present are such as to make it impossible to specially encourage outside investigators to come to the station for the prosecution of their special studies, still several who were willing to take their chances of finding enough to make the coming worth their while have been at La Jolla during the summer.

Dr. David Marine, of the medical research laboratories of the Western Reserve University, devoted six weeks to studying the endostyle of the lower chordates. His aim is to apply chemical and physiological tests to this structure for the purpose of finding whether any of the reactions characteristic of the thyroid of man and the higher chordates, can be detected. It was not possible in so short a time to carry the work to definite results, positive or negative. It is certainly to be hoped that Dr. Marine may be able before long to push the study to a conclusion.

Professor H. B. Ward, of the University of Illinois, with three graduate students, spent about six weeks at the station on the very laudable mission of gaining a knowledge of the marine fauna and general biological conditions of the region. Incidentally Dr. Ward gave two popular lectures to appreciative audiences of La Jolla citizens, one on "Zoology in Relation to Human Welfare," and the other on "Public Health."

WM. E. RITTER

LA JOLLA, CAL.,
October 3, 1911

THE INTERNATIONAL EUGENICS CONGRESS

THE Eugenics Education Society has arranged for an International Eugenics Congress to be held in London from July 24 to 30, 1912, under the presidency of Major Leonard Darwin. It is proposed to group the papers into the following four sections:

1. *The Bearing upon Eugenics of Biological Research.* Facts of Heredity; Physiological Aspects of Heredity; Variations, their Nature and Causation; Race Mixture.
2. *The Bearing upon Eugenics of Sociological and Historical Research.* Historical Evidence with regard to changes in Racial Characters; Birth-rate and Death-rate Statistics; Effects of Medical and Surgical Treatment in Encouraging Unfitness.
3. *The Bearing upon Eugenics of Legislation and Social Customs.* Marriage Laws and Customs; Taxation; Economic Conditions; Insurance; Trades Unionism.
4. *Consideration of the Practical Applications of Eugenic Principles.* Prevention of the Propagation of the Unfit by Segregation and Sterilization; Voluntary Restriction of Propagation of the Unsound; The Encouragement of the Propagation of the Fit; Promulgation of the Eugenic Ideal; The Place of Eugenics in Educational Systems.

PROFESSOR JOHANNSEN'S COLUMBIA LECTURES

Professor Johannsen's lectures on the modern principles of heredity at Columbia University, briefly announced in a previous issue of Science, will deal with the following topics:

October 13—"The Problem of Personal Characters." Old Conceptions of Heredity; Transmission of Personal Characters; Variation and Phenotypes; Selection; Galton's Law of Regression; The Study of Pure Lines; Explanation of Galton's Regression; Criticisms of the Transmission Conception; The Genotype Conception; Personal Characters Irrelevant; Constancy of Genotypical Elements; Modern Definition of Heredity.

October 20—"The Problem of Unit Factors." Homozygotes and Heterozygotes; Hybridization; Mendelism; Segregation and Combination of Genotypical Elements; Genetic Constructions; Personal Characters as Reactions of Genotypical Elements; Complicated Cases.

October 27—"Problems of Correlation and Sex." Exceptions to Mendelian Behavior; Blending Characters; Repulsion and Coupling of Genotypical Elements; Sex-limited Inheritance; Cytological Questions.

November 3—"The Problem of New Biotypes." Mutations; External Factors acting upon Genotypical Elements; Mutations and Mendelism; Acquired Personal Characters; Criticisms of Lamarckian Views; Facts and Speculations; Morphological Views *versus* Chemical Views in Genetics; Summary.

These lectures are given under the auspices of the departments of botany and zoology and will constitute the twelfth series of Columbia biological lectures. They will be given in Room 305, Schermerhorn Hall, at 4:10 P.M., and are open to the public.

SCIENTIFIC NOTES AND NEWS

By invitation of the trustees of the New York Public Library the autumn meeting of the National Academy of Sciences will be held in its new building, Forty-second Street and Fifth Avenue, beginning on Tuesday, November 21. The first business meeting of the academy will be held on Tuesday morning at eleven o'clock and the first open scientific session will be at two o'clock in the afternoon. Titles of papers should be sent to Dr. John S. Billings, secretary of the local committee, New York Public Library.

Owing to the epidemic of cholera, the various international congresses, geographical, agriculture and tuberculosis, will not meet in Rome this autumn. They have been postponed until the spring of 1912, the exact date not yet being determined.

Among the degrees given by the University of Vermont on the occasion of the installation of President G. P. Benton were doctorates of laws on Dr. Henry S. Pritchett, president of the Carnegie Foundation; Dr. E. F. Nichols, president of Dartmouth College, and Dr. Harvey W. Wiley, chief of the Bureau of Chemistry.

Professor A. A. Michelson, head of the department of physics, at the University of Chicago, has returned from the University of Göttingen, where he has been exchange professor during the summer semester.

Dr. C. Willard Hayes, chief geologist of the U. S. Geological Survey, has retired to engage in technological work in Mexico.

Dr. Edward Renouf, collegiate professor of chemistry at the Johns Hopkins University since 1890, has retired from active service.

Dr. Franz Mertens, professor of mathematics at Vienna, has retired from the active duties of his chair.

Associates of the Harvard University Museum for five years from September 1, 1911, have been appointed as follows: Robert Tracy Jackson, S.D. (paleontology); Frank Springer, Ph.B. (paleontology); Frank Shipley Collins, A.M. (botany); Edward Phelps Allis, Jr., LL.D. (zoology); Arthur Cleveland Bent, A.B. (ornithology); William Barnes, S.B., M.D. (entomology); Frederick Blanchard (entomology); Andrew Gray Weeks, A.B. (entomology).

Dr. L. R. Geissler, of Cornell University, has been appointed associate psychologist in the physical laboratory of the National Electric Lamp Association, Cleveland, O.

Professor Luigi Carnera, director of the International Latitude Stations at Carloforte and Oncativo, has been appointed professor of

astronomy and geodesy in the Instituto Idografico della R. Marina at Genoa.

DR. EMIL HEINRICHER, professor of botany at Innsbruck, has been elected a corresponding member of the Vienna Academy of Sciences.

DR. RICHARD C. MACLAURIN, president of the Massachusetts Institute of Technology; Professor Arthur A. Noyes, of the department of chemistry; Professor Charles R. Cross, of the department of physics; Professor C. M. Spofford, of the department of civil engineering; Professor C. H. Peabody, of the department of naval architecture, and Professor Dougald C. Jackson, of the department of electrical engineering, have returned from visits made abroad during the present summer to inspect foreign schools of technology and laboratories, in view of the new buildings to be erected by the Massachusetts Institute of Technology.

WE learn from the *Journal* of the American Medical Association that the New York Post-Graduate Medical School and Hospital is preparing to send out an expedition next spring for the purpose of studying tropical diseases. A special fund of $15,000 for this purpose has been given by Col. Robert M. Thompson, of New York, and John H. McFadden, of Philadelphia. Capt. Joseph F. Siler, M.C., U. S. A., lecturer on tropical diseases, will have charge of the expedition.

PROFESSOR A. W. JOHNSTON, of the department of geology of the University of Minnesota, spent the summer in a section of northern British Columbia within the arctic circle heretofore unexplored.

PROFESSOR EDWARD LEE HANCOCK, professor of applied mechanics in the Worcester Polytechnic Institute, died on October 1, at the age of thirty-eight years.

THE Rev. Mariam Balcells, member of the Jesuit order, professor of mathematics at Boston College for the past two years, previously director of the department of solar physics at the Observatorio del Ebro, Tortosa, Spain, died on October 2, at the age of forty-seven years.

DR. N. V. USSING, professor of mineralogy in the University of Copenhagen, has died at the age of forty-seven years.

M. GIROD, professor of botany at the University of Claremont, has died at the age of fifty-six years.

THE death is also announced of Dr. Karl Waitz, professor of physics and astronomy at Tübingen.

THE Bureau of Mines will hold a national mine safety demonstration at Pittsburgh on October 30 and 31. The program is as follows:

October 30—9:00 A.M. to 12:00 M.: Demonstration and explosions; exhibit of explosives, safety lamps, fuel testing, etc., at the Bureau of Mines buildings, in the Arsenal Grounds, Fortieth and Butler Streets, Pittsburgh, Pa. 2:30 P.M. to 5:00 P.M.: Demonstrations and explosions in the Bureau of Mines Experimental Mine near Bruceton, ten miles south of Pittsburgh, Pa., reached by Baltimore and Ohio R. R. train, leaving Pittsburgh at 2:00 P.M., eastern time.

October 31—9:00 A.M. to 10:30 A.M.: First aid exhibit. 10:30 A.M. to 11:30 A.M.: Explosion, rescue work and mine gas. 11:30 A.M. to 12:00 M.: Presentation of prizes by President Taft. 12:30 P.M.: Luncheon to President Taft at the Hotel Schenley. 2:00 P.M. to 5:00 P.M.: President Taft will review river pageant. 7:30 P.M.: Dinner to President Taft at the Hotel Schenley.

A REUTER message from Adelaide states that Mr. Brown, the South Australian government geologist, reports that the uranium ores recently discovered in the northern portion of South Australia possess great importance.

THE world's production of quicksilver last year was 3,747 short tons, of which the United States produced 773 short tons. Quicksilver is usually quoted in "flasks," a flask containing 75 pounds. The American production therefore represents 20,601 flasks. Of this amount California furnished 17,211 flasks. In 1850 the quicksilver production of that state was 7,723 flasks, but California's greatest production was in 1881, when the yield was 60,851 flasks. In 1910 only two countries produced more quicksilver than the United States —Italy 882 tons and Spain 1,102 tons. These

and other statistics are given in an advance chapter on quicksilver from "Mineral Resources of the United States," 1910, by H. D. McCaskey, of the United States Geological Survey.

During the past few years various expeditions from the University of Chicago have secured from the fossil fields of northern Texas the largest and best collection of Permian vertebrate fossils in the world. The past summer an expedition from the university, under the direction of Professor S. W. Williston, has explored the Permian deposits of northwestern New Mexico with valuable results. These Permian deposits, of small extent, in Rio Arriba County, were discoverd more than thirty years ago, but have been neglected by explorers ever since, and their precise location even was unknown to geologists. As a result of Professor Williston's excavations, numerous fossils have been shipped to the university, many of which are unknown to science. This collection includes six or seven new genera of reptiles and amphibians, one of which is represented by one of the most perfect skeletons, about six feet in length, ever found in any deposit in America. This skeleton will be mounted in Walker Museum the coming year by Mr. Paul Miller, who collected its parts. The scientific results of the expedition will shortly be published in detail.

A conference of the International Aviation Map Commission took place recently in Brussels under the presidency of Prince Roland Bonaparte. In the proceedings of the conference as reported in the *British Geographical Journal*, a distinction was made between questions ripe for discussion and those of a more problematical character; definitely formulated votes being adopted only in the case of the former. They arose for the most part directly out of the discussions of the conference, while the more theoretical matters were the subject of lectures by specialists. The recommendations adopted had to do with (1) scale; (2) subdivision of the map and boundaries of the several sheets; (3) the numbering and naming of the sheets; (4) the orthography of geographical names. An important decision was reached in the adoption of the scale of 1:200,000, while it was also agreed that the separate sheets should each embrace the field of one degree. The initial meridian to be that of Greenwich, the descriptive text in the language of the nation concerned. A single sheet of the International millionth map would correspond to 25 sheets of the new aviation map, but considerable objection was made to the employment of the former for the purpose of giving a general view of the area in question. As regards the method of representing the surface, the only recommendation that found acceptance (out of some twenty-five different suggestions) was that on the occasion of the next great overland aviation contest, a variety of such systems might with advantage be tested. Exception was taken by the English representative to overloading the map with conventional signs in red; stress was, however, laid on the desirability of a uniform representation of electrical power stations by series of red crosses. The question of the representation of relief was also held to be not yet ripe for decision. On this subject a paper was read by Dr. K. Peucker, one of the Austrian representatives, who insisted on the need of a special method of representation fitted to bring out visibly to the eye the extent of the difference in altitude, so that the aviator might be able to grasp the exact measure of the obstacles to free progression. In accordance with this view, it has been decided by the German Association of Aviators to construct a specimen-map of a portion of the Rhine basin on the color-plastic system. The recommendations of the conference, as also the results of experiments in the desired directions, are to be laid before the International Aviation Congress at Rome in October. Special mention should be made of an address by Professor Berget before a general meeting of the Belgian Aero Club on the subject of topography and aeronautics. The lecturer brought out on the one hand the reasons which lead aeronauts to make a new claim on cartography, and on the other, the extent to which aviation may help to raise cartography to a higher level.

OVER $3,000,000 worth of abrasive materials were produced in the United States last year. All branches of the abrasive industry showed notable growth except the millstone and the grindstone industries, according to W. C. Phalen, of the United States Geological Survey, in an advance chapter from Mineral Resources of the United States, 1910. The total estimated value of all the abrasive materials consumed in this country last year was $4,234,662, of which $3,010,835 worth were of domestic production. Abrasive materials may be divided into two classes—natural and artificial. The production of artificial abrasives has shown great increase since they were first made, less than 15 years ago, and at the present time it exceeds that of the natural abrasives. During 1910 natural abrasives valued at $1,406,805 were produced in 21 states. Of these materials, grindstones and pulp stones led with a production valued at $796,294. The production of burrstones and millstones in the United States in 1910 was valued at $28,217. The production of oilstones and scythe stones amounted to $228,694, compared with $214,019 in 1909. Garnet is one of the very hard minerals, and is extensively used as an abrasive. The production of abrasive garnet in 1910 amounted to 3,814 short tons, valued at $113,574. This was an increase of 842 tons, or 28 per cent., in quantity, and of $11,259, or 11 per cent., in value. In the class of artificial abrasives are included carborundum, alundum and crushed steel. The production of artificial abrasives in 1910 showed an increase of 2,559,000 pounds in quantity and of $238,210 in value, as compared with 1909.

UNIVERSITY AND EDUCATIONAL NEWS

THE class of 1886 has presented to Harvard University $100,000, the income of which is to be used for the benefit of the college. From Mr. William J. Riley, of Boston, the university has received $25,000 for the establishment of scholarships in memory of his nephew Clemen Harlow Condell.

DR. GUY POTTER BENTON was installed as president of the University of Vermont on October 6. In the morning there were addresses from representatives of various institutions; in the afternoon the governor of Vermont administered the oath of office and Dr. Benton gave the inaugural address. In the evening there was a corporation dinner and on the preceding day an educational conference was held.

MESSRS. J. B. DUKE and B. N. DUKE have made further gifts amounting to $228,000 to Trinity College.

THE installation of Dr. Elmer Ellsworth Brown as chancellor of New York University will take place on Thursday, November 9. Presidents Lowell of Harvard, Hadley of Yale, Butler of Columbia, Schurman of Cornell and Finley of the New York City College will be among the speakers.

PROFESSOR JAMES WILLIAM TOUMEY, who has acted as head of the Yale Forest School during the absence of Professor Graves, has been elected director for the year 1911–12.

WILLIAM H. EMMONS, associate professor of economic geology and mineralogy at the University of Chicago, and geologist for the United States Geological Survey, has been appointed head of the department of geology at the University of Minnesota.

A. C. TROWBRIDGE, instructor in geology at the University of Chicago and assistant geologist of the Illinois Geological Survey, has been appointed professor of geology at the State University of Iowa.

As a result of the resignations of Professor R. A. Harper, Dr. W. G. Marquette, assistant professor, and A. B. Stout, instructor, in the botany department of the University of Wisconsin, who have accepted places at Columbia University, the regents of the state university have appointed E. M. Gilbert, assistant professor of botany; W. N. Steil, E. T. Bartholemew and Alban Stewart, instructors in botany, and A. G. Johnson, assistant in botany.

AT the University of Wisconsin E. Baumgartner has been appointed instructor in anatomy and Assistant Professor Bennett M. Allen

has been transferred from the department of anatomy to that of zoology. New assistants appointed in zoology are H. V. Lacy, Edward H. Jones, Elizabeth A. Smith and Nathan Faster.

Dr. Robert Retzer, of the University of Minnesota, has been appointed assistant professor of anatomy in the University of Chicago.

A. B. Dunning, S.B. (Harvard, '07), has been appointed assistant professor of mathematics at Boston University.

At Northwestern University Leon Irwin Shaw, Ph.D. (Wisconsin), has been appointed instructor in chemistry; George Vest McCauley, Ph.D. (Wisconsin), instructor in physics, and Chester Henry Yeaton, A.M. (Harvard), instructor in mathematics. Robert Harvey Gault, Ph.D., has been advanced from an instructorship to an assistant professorship in psychology and has been appointed editor of the *American Journal of Criminology*.

DISCUSSION AND CORRESPONDENCE

THE COTTON WORM IN MASSACHUSETTS

During the last week in September of the present year, a number of moths of the cotton worm, *Alabama argillacea* Hübn., were captured at Amherst, Mass., some of them being taken at night, while others were found at rest during the daytime. Although this insect has been taken at Amherst before, there are no records of it in any such abundance, and it would seem that there must have been quite a pronounced northern migration of this species this season. The moths were very fresh and perfect.

There have been occasional captures of this moth in the New England states, and in the collection of Mrs. C. H. Fernald is a fresh pair taken in September (probably 1881) at Orono, Maine.

It may be well to call attention here to a discussion on the habits of this insect at a meeting of the entomological members of the American Association held in 1882, and reported in *The Canadian Entomologist*, Vol. XIV., p. 151, where some evidence is given, supporting the view that the appearance of this species in the north is not, at least in all cases, due to migration.

H. T. Fernald

VECTORIAL TREATMENT OF SECONDARY MAXIMA IN GRATING SPECTRA

To the Editor of Science: A friend has been good enough to direct attention to a regrettable error in my review of Wood's "Physical Optics," which appeared in Science, September 29, 1911.

Instead of alluding to "the author's clever vectorial treatment of secondary maxima in grating spectra," I should have called attention to the fact that the essential features of this beautiful geometrical and graphical method were invented by Professor Arthur L. Kimball. The omission of this fact from Professor Wood's text is doubtless owing to want of space.

I still remember the delight with which I read Professor Kimball's paper when it appeared in the *Philosophical Magazine*, July, 1903, and can explain my forgetfulness and inadvertency only as a consequence of the very considerable amount of sand which has run through my hour glass.

Henry Crew

QUOTATIONS

THE UNIVERSITY PRESIDENT AND HIS PROFESSORS[1]

A successful college or university president can not afford, for the sake of his own success, to make his administration in any sense a personal one. It is his business to see to it that the students who commit their training to the institution he serves, are provided with the very best teachers and lecturers the funds at his command will allow him, with the consent of his board of trustees, to give to these young people. If there are instructors whose worth has been demonstrated by years of service, he will put forth every possible effort

[1] Extracts from an address to the senate of the University of Vermont and State Agricultural College by Guy Potter Benton, installed as president on October 6, 1911.

to retain them. The president who has the welfare of his institution at heart will spare no effort to secure the permanency of tenure of those on his teaching staff who are desirable. If here and there he finds a colleague whose work is not satisfactory and can not be made so, he will meet the situation fearlessly in the interests of the young people committed to his care, but he will also meet it with a thoughtful regard for the feelings of the colleague concerned. A resignation is always less painful than a dismissal. It tries the courage of a manly president more to ask, in the spirit of kindness, a resignation than it does in the presence of his board to demand, with heartlessness, a dismissal. The unpleasant responsibility will be courageously accepted by the high-minded man and in a fraternal spirit, the unsatisfactory teacher will be approached by his president months before his connection with the college must be severed with a courteous request for his resignation. An instructor of good sense will appreciate the consideration that prevents a humiliating dismissal, and that affords him ample time, while still under pay, to find another position; and his resignation will be given as requested without disturbance. He who lacks this fine sense of appreciation will still be dealt with in fearless kindness by his president and will not be retained at the expense of institutional efficiency. . . .

If one feature of presidential duty may be emphasized at the expense of another, it will doubtless be agreed that the chief responsibility of a college president is for his educational staff. Before boards of trustees came to a proper comprehension of their limitations, they took official notice of the fitness or unfitness of every member of the faculty, and not only determined the retention or dismissal of incumbent professors and instructors, but solemnly debated the qualifications of all proposed candidates before voting to fill a chair. Their opinion of the worthiness of a professor to continue was formed by the report concerning him coming from immature students or some other incapable informant. As to the election of new faculty members, the board was governed in most instances by flatteringly worded and usually worthless testimonials. To-day it would be difficult to find a trustee presumptuous enough to entertain the thought of passing judgment on the qualifications of teachers. The president is charged with this responsibility and the head of an institution must stand or fall on his ability properly to meet this responsibility. The retention of present members of his faculties and the election of new members in the modern university depends entirely upon the dictum of the president. Those who object to granting such arbitrary power to one man will, on reflection, admit that to hold an executive responsible for all the work of an institution, including the teaching done, would be unfair unless there were guaranteed therewith the privilege of choosing the colleagues for whose work he must answer. In some instances, the president is required by ordinance to nominate new faculty members, the Board confirming or rejecting his nominations, and that is the system which will obtain in this institution from this time forward until it is changed by order of the board of trustees. . . .

The wide-awake president may know of the competency or incompetency of his colleagues by ways more accurate than personal inspection can guarantee. The college community is much more compact than a large public school system. The professors do their work in class-room, lecture-room, laboratory, library and study in buildings on the same grounds and near to each other. The president, when at home, is constantly in their midst and with his hand ever on the college pulse, knows more, or should know more, of what his associates are thinking and accomplishing than the public school superintendent knows of his teachers after all his inspection. The daily intercourse of the president with his coworkers in faculty and committee meetings, in private conferences, and in social relationships, will give to the keen leader of men a knowledge which will enable him to make a fair judgment of individual educational fitness in the day of final reckoning.

The administrative office is a veritable cess-

pool where unpleasant experiences are deposited. All complaints are left there, and if the president, as a spiritual chemist, is skilful in filtering, the residuum will reveal to him the actual substance of all that is justly chargeable against his complained-of colleagues. . . .

No one will question the right of faculty members to advise the president. If he is as wise as such an official should be, he will seek the counsel of his associates and, knowing that "in the multitude of counsellors there is wisdom," he will be ready to modify his plans and policies after hearing from his colleagues. The right to advise, however, does not include within it the prerogative of censorious criticism on the part of the colleagues of the president. Next to a despotic egotist in the presidency, the most obstructive hindrance to the growth of a healthful spirit in a given institution is a coterie of professors, painfully sychophantic in the presence of their "lord and master," and bitterly denunciatory of him when left to themselves. It is difficult to conceive of a more painful caricature on true manhood than that made up of a little professorial group gathered together in a darkened corridor or behind a building gesticulating wildly against the administration, unless it be the same small crowd in the study of one of the number, or in some club-room, planning surreptitiously for the overthrow of their chief.

The president of one of the larger state universities of the central west was apparently highly esteemed by all those who served with him, but when he resigned, one of the prominent professors, too cowardly to be other than obsequious while he thought the tenure of the president permanent, remarked, "Well, there is certainly a great ground-swell of relief among the faculty, now that we are to be relieved from the incubus of this administration." Such reprehensible hypocrisy by those who teach can not but exert a blighting influence upon the life of the institution. . . .

In order that the work of administration might be as efficient as possible, at the recent annual meeting of the board of trustees, I requested the authority to appoint a committee on efficiency, consisting of certain members of the board of trustees and of the teaching body. This privilege was granted. The committee appointed has begun its investigations It is proposed to ascertain, as soon as possible, just how much work each member of the educational staff is doing in the matter of instruction, how much outside work of a public character he is doing for the benefit of the institution, what he is producing in connection with the literature of his chosen line of specialization and—in short—to determine his value to the institution as compared with that of his colleagues. These investigations may result in the conclusion that some men are doing too much work and that others are not doing a sufficient amount. They may lead, as a consequence, at the end of the year to the merging of some positions and to the abolition of certain others. On the other hand, these investigations may lead to a division of work and to the establishment of new positions. A due regard must always be given to the rights of the individual, but the interests of the institution must be made paramount to those of any individual on the educational staff. I feel quite sure that all of you who take the broad and statesmanlike view of educational obligations will agree with the statement just made. No changes could be made now with any degree of intelligence that would command the respect of the college world. The status quo will therefore be maintained for the present. Since, however, a working basis is necessary, some little organization is imperative from the outset and a few important facts must be recognized. . . .

The vulgar swagger assumed by some university and college professors in this latter day would be pitiable if it were not positively mischievous. Time was when the man who taught in college believed that his life should be one of consecration to the highest ideals of character. He believed that all questionable conduct should be avoided. For the sake of his influence upon his students he consistently refrained from indulging himself in those

diversions which to men occupying less responsible positions might be occasionally allowable if not always permissible.

No more hateful spectacle confronts advancing civilization than a beer-sipping, wine-bibbing college or university professor. He is hateful because he is incongruous. More than that, he is hateful because of the havoc he works as an iconoclast in the beautiful temple of youthful ideals. It is a safe prediction in the near coming day when the American saloon is only a historic tradition, that the college professor who drinks in public or in private will not be tolerated beyond the meeting of the board of trustees next succeeding his discovery, and I should say to you in perfect candor at this time, in order that there may be no misunderstanding from the beginning, that I will not serve on a teaching body with any man who uses intoxicating liquors in any form whatsoever. My responsibility to young manhood and womanhood for character ideals is too great to permit me to attempt to bear the burden of responsibility which I could not escape for a colleague who leads an immoral life.

SCIENTIFIC BOOKS

The Mutation Theory. Volume II. The Origin of Varieties by Mutation. By HUGO DE VRIES. English translation by Professor J. B. FARMER and A. D. DARBISHIRE. Pp. viii + 683. Six colored plates and 149 text-figures. Chicago, The Open Court Publishing Co. 1910.

In May, 1910,[1] the writer had the honor of reviewing Volume I. of the English translation of "Die Mutationstheorie." The inspiration which this volume brought to a large circle of readers made the appearance of volume two doubly welcome. The careful study of the first volume introduced many workers for the first time to the author's own statement of the essentials of the mutation theory, and these essentials, together with a brief summary of de Vries's many important and positive contributions to theoretical and practical biology, through this and his numerous other related writings, were given in the above mentioned review.

Of volume two of the translation, Part I. includes Band I., Abschnitt 4, of the original, treating of The Origin of Horticultural Varieties, with chapters on The Significance of Horticultural Varieties in the Theory of Selection, Latent and Semi-latent Characters, The Different Modes of Origin of New Species, The Sudden Appearance of and the Constancy of New Varieties, Atavism, Experimental Observation of the Origin of Varieties, Non-isolable Races, and Nutrition and Selection of Semi-latent Characters.

Part II., The Origin of Eversporting Varieties, includes, from the original, Band II., Abschnitt 2, IV., The Origin of Eversporting Varieties, with four chapters on: I., Tricotylous Races (the title of the German original is "Kreuzung tricotyler Rassen"), omitting § 24 (Kreuzung der Mittletassen mit den Halbrassen) and § 25 (Kreuzung tricolyler Rassen von verschiedenen Arten); II., Syncotylous Races (Kreuzung syncotyler Rassen), omitting § 31 (Kreuzungsversuche); III., The Inconstancy of Fasciated Races (= Band II., Absch. 5, IV.), and IV., Heritable Spiral Torsions (= Band II., Absch. 5, IV.).

Part III. includes Band II., Aschnitt 6, treating of The Relations of the Mutation Theory to other Branches of Inquiry, embracing four chapters: I., The Conception of Species According to the Theory of Mutation; II., The Range of Validity of the Doctrine of Mutation; III., The Material Vehicles of the Heredity Characters; and IV., Geological Periods of Mutation.

Those portions of the original work treating of hybridization (including § 31 mentioned above, and Band II., Abschnitt 1, and Abschnitt 2, I.–III.) have been omitted from the first two volumes of the English translation. Thus the second volume is not merely a translation of volume two of the original. It covers largely the same ground as de Vries's English lectures, published under the title of "Species and Varieties, their Origin by Mutation," but has the advantage of illustrations, which were lacking from "Species and Varieties."

[1] *Science*, XXXI., 740–743, 1910.

The publication of volume two will help to dispel the erroneous, but still quite prevalent, notion that the theory of mutation is based entirely on observations of the evening-primrose. Even one who has himself carried on experimental pedigreed cultures can not but admire the thoroughness and patience with which thousands of individual plants of scores of different species were examined with minutest care in order to insure adequate grounds for inductive inference, or to secure sufficient data for the confirmation or rejection of a hypothesis. The conception and elaboration of this hypothesis of mutation is the most Darwin-like performance since Darwin, and is, without doubt, one of the most important fruits of Darwin's labors. Like the source of its inspiration (the "Origin of Species"), "Die Mutationstheorie" has given color and direction to all lines of biological inquiry, which will persist for decades to come.

As in volume one, so also throughout volume two, the author keeps close to Darwin's Darwinism, and insists that, far from being intended to supplant the theory of selection, as Darwin held it, the mutation theory is intended merely to complement the other. Thus on page 609: "To Darwin's mind the essential point was . . . that natural selection is a sieve. . . . It creates nothing, as is so often assumed; it only sifts. . . . How the struggle for existence sifts is one question; how that which is sifted arose is another." It is really difficult to conceive how careful readers could ever have confused the issue on this point. Any attempt to restate in this periodical the essence of the mutation-theory would now be out of place.

That phase of de Vries's philosophy which has perhaps met with the greatest opposition is his hypothesis of intracellular pangenesis. It has been rejected as too mechanical and too formal, one author implying that pangens are more "mechanical" than atoms and molecules! This is not the place to discuss the validity of intracellular pangenesis, but it must surely be recognized that as a working hypothesis it has fully justified itself, since it lies at the bottom of all of de Vries's experiments, and of his own explanation of the results. Putting aside the question of its heuristic value, its framer says (p. 643): "for myself pangenesis has always been the starting point of my inquiries; at first only in a theoretical way, but afterwards also for the experimental investigations described in this book. Especially is it this hypothesis which has led me to search for mutations in the field." It is doubtful whether the field observations which led to the classical experiments with Lamarck's evening-primrose and thus to the actual observation of the origin of elementary species, would ever have been made if the hypothesis of intracellular pangenesis had not first taken form in our author's thought (see footnote 3, p. 643, and "Intracellular Pangenesis," English edition, p. 74, footnote).

Whether we accept intracellular pangenesis as an expression of truth or not, and even if we reject it as a working hypothesis, a clear understanding of it is absolutely essential in order to interpret the theory of mutation as it exists in the mind of de Vries. It was largely for this reason that the present writer thought it worth while to translate "Intracellular Pangenesis" into English.

In the language of intracellular pangenesis:

1. Premutation consists in the formation of a new pangen.

2. Progressive mutation consists in premutation, *plus the initial activation of the new pangen.* The result is a new elementary species.

3. Retrogressive mutation is the reverse of progressive mutation; it consists in the return of a pangen from an active to a latent condition. White-flowered varieties are thus caused.

4. Degressive mutation consists in such a transposition of pangens that either (*a*) the more recently activated pangens become semi-latent, being active in rare instances only, thus giving rise to a *half-race* (*e. g.*, wild four-leaved clovers, and other teratological forms); or (*b*) the active pangens become semi-active, giving rise to ever-sporting varieties, middle races (*e. g., Trifolium pratense quinquefolium*

(five-leaved clovers), *Plantago lanceolata ramosa*, and variegated leaves).

"If one of the two parents [in a cross] stands in the relation to the other of having arisen from it by retrogressive or degressive mutation," the progeny, in successive generations, follow Mendel's laws; otherwise they do not, and the result of a cross is then a unisexual, or constant hybrid race. Constant hybrid races correspond to progressive mutations, the Mendelian law to retrogressive and degressive forms of differentiation (pp. 576–577).

But though the mutation theory is a direct outgrowth of the hypothesis of intracellular pangenesis, it fortunately does not stand or fall with the latter, for no scientific theory ever had a firmer foundation in fact—in experimental evidence—than that of mutation.

De Vries claims to have demonstrated experimentally that: (1) "Ordinary or fluctuating variability does not provide material for the origin of new species" (p. 56). While this does not exclude the possibility of different modes of origin of new species, still "Inferences drawn from data after its appearance can hardly be considered as decisive" (p. 56). That is, *the problem is an experimental one.* (2) "The elementary species are demonstrably the existing units of the system; whilst the larger species are only aggregations of these." (3) Elementary species (the theory has nothing to do, except indirectly, with the mode of origin of taxonomic groups) do arise by the method of mutation (discontinuous variation). They have time and again, in a wide variety of sorts, been seen so to arise. No one has ever actually witnessed the origin of a species by any other method.

It is probable that the doctrine of the elementary units of organisms will ultimately prove to be de Vries's most important contribution; though perhaps this doctrine means less to the advancement of philosophical and experimental biology than the firm establishment of the fact, quoted on the fly-leaf of "Species and Varieties," that "The origin of species is an object of experimental investigation." The demonstration of this fact, so conclusively as to compel practically every investigator to acknowledge its truth, is the greatest service rendered to evolutionary biology since Darwin.

On the whole, the translators have performed their work well, though in a few places (*e. g.*, on pp. 608 and 609, cf. German edition, Vol. II., pp. 666), the English can hardly be regarded as a translation of the original. The color plates and the text-figures, notably plate IV., and figures 26, 40 and 54, are quite inferior to those of the German edition. However, it is not easy to find points to criticize adversely, and every one is hoping that an English edition of the now omitted portions, by the same translators, will soon appear.

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Outlines of the Theory of Electromagnetism. By GILBERT T. WALKER. Cambridge, University Press. 1910.

This volume contains a series of lectures delivered before the Calcutta University on some of the more important developments of electromagnetic theory. The chief novelty of it, compared with other English books on mathematical physics, lies in the consistent use of vector methods, and their advantage is shown by the large amount of material condensed into fifty-two pages. While little of the material is new, the book will be of great assistance to those who wish to familiarize themselves with the present condition of the theory, as well as to those who wish to obtain a working knowledge of vector methods applied to physical problems. For the latter object no better course could be devised than a careful study of this book, with frequent transformations of the vector formulæ into their more familiar Cartesian equivalents.

The first chapter gives an outline of vector analysis, including the vector expressions for the more important analytical theorems of constant use. In the second chapter vector methods are applied to magnetostatics, and

here the advantages of these methods are most clearly brought out. The third chapter gives a statement of the Hertzian form of Maxwell's equations. In Chapter IV. Hertz's theory for moving media is discussed and it is shown how experimental results prove its inadequacy.

The motion of a single charge moving with uniform velocity through the ether is considered in Chapter V., and in the next chapter the electron theory of Lorentz is applied to stationary media. The treatment of stresses within a material medium is not satisfactory; no account is taken of the variation of specific inductive capacity with the state of strain, and therefore the stress system obtained is that of Maxwell, which we know is not capable of experimental verification. In the last chapter Lorentz's theory is applied to moving bodies, ending with a brief account of aberration. The interpretation of the Lorentz transformation in terms of the theory of relativity is not touched upon.

There are many other matters that might properly come within the scope of this work, but it does not profess to be exhaustive, and as an outline it may be commended most highly.

E. P. Adams

CONTAGIOUS ABORTION IN CATTLE

It is often asserted that tuberculosis is, from the economic point of view, the most important disease affecting the cattle of this country. It is undoubtedly true that contagious abortion is to be ranked as second in economic importance and, by many of the best informed breeders and dairyman this disease, that may destroy the usefulness of a dairy animal during one fourth to one half of the average period of productivity, is considered more important than tuberculosis. Certainly it is true that the breeder and dairyman have been more helpless in the fight against this disease than against tuberculosis, for knowledge is available that will enable any farmer to free his herd from tuberculosis and so to maintain it. In the case of contagious abortion, no such knowledge is available, and until quite recently no method had been devised by which it could be ascertained whether or not an animal about to be introduced into a healthy herd was infected. The work of Bang and his associates has demonstrated the cause of the disease as it appears in Denmark. This work has been confirmed by others in England and Germany. In the United States, in spite of the efforts of many investigators, the *B. abortus* of Bang had not been found. Dr. W. J. MacNeal, formerly at the University of Illinois, isolated an organism that he believed, relying on its cultural and morphological characteristics, to be identical with the Bang organism.

Within the last year, the complement deviation test, now so widely used for the diagnosis of syphilis, has been applied with great success to the detection of contagious abortion in cattle, by Drs. Bang and Holth in Copenhagen. Dr. W. P. Larson, who had become familiar with the test during his association with the Danish bacteriologists, returned to this country in May, since which time he has been engaged in connection with the departments of agricultural bacteriology and veterinary science of this experiment station in the study of the disease in this country. Using a culture of the organism brought from abroad as one of the specific components of the test, it has been shown that the disease as it occurs in this country is caused by the same organisms as found in Europe. Using the blood serum of known infected animals, the complement deviation test can be employed to identify a suspected organism. The organism has been isolated from fetuses from five herds in various parts of the state and the identity of the cultures established by the test. There remains no doubt that the disease in this country is caused by the same organism as that found in Europe.

H. L. Russell,
Director
Agricultural Experiment Station,
Madison, Wis.,
September 1, 1911

SPECIAL ARTICLES

A COUNTING METHOD FOR THE MECHANICAL ANALYSIS OF SOILS[1]

MANY investigators have worked on the problem of the mechanical analysis of soil, notably, Osborne,[2] Hilgard,[3] Hopkins,[4] Briggs,[5] Yoder[6] and Atterberg,[7] and the final result seems to indicate that some form based on sedimentation in water is the most serviceable for practical purposes. However, it may be of interest to soil workers to note a counting-plate method which approaches the problem from a different angle.

The counting-plate method has been used for the determination of soil particles in an attempt to save the time of centrifuging and evaporating the silt and clay suspensions. The sand is obtained by subsidence as in the regular method. The total weight of the silt and clay is determined by difference. The relative amounts are then obtained as follows:

The method is based on counting the number of silt and clay particles on a counting plate and from the relation thus established to determine the amount of clay and silt in the soil. This is an adaptation of a method employed in many different lines of work where both the number and diameter of small grains are to be obtained; as in the examination of blood, starch, etc. This method is not recommended for general use with soils, and should be used only where time is especially important or the facilities are not available for the determination of silt and clay as described above.

A definite amount of soil is weighed out, put in a sterilizer bottle with water and ammonia, and shaken in a mechanical shaker for at least seven hours. With most soils one half gram material and 120 cubic centimeters water give a good dilution for accurate counting. A compound microscope with a micrometer eyepiece and a counting plate are necessary. In the micrometer used one scale division corresponds to 0.005 millimeter, the superior limit of the clay, and ten divisions to 0.05 millimeter, the superior limit of the silt. The counting plate is marked off in squares of 0.1 millimeter a side. After removing from the mechanical shaker, the contents of the bottle are thoroughly shaken by hand and a sample for examination immediately taken from the center of the bottle. A drop is placed on the counting plate and the number of silt and clay particles in ten squares counted. The sand need not be considered. In most cases it settles quickly and escapes being taken in the subsample. If a sand particle appears in the subsample it is disregarded.

As one silt particle is much heavier than one clay particle, a factor must be used in obtaining a ratio to express the relative total weights of silt and clay in the soil. This factor was determined approximately by comparing the results obtained by counting with those obtained from analyses made in the regular way. For instance, soil No. 5,862 gives a count in ten squares of 26 silt particles and 2,020 clay particles. The regular analysis gives 23.06 per cent. silt and 45.78 per cent. clay, practically twice as much clay as silt. So first the number of silt particles (26) is multiplied by 2, giving 52. Dividing the number of clay particles (2,020) by 52 gives the factor 38 plus, or shows that 38 clay particles weigh as much as one silt particle. Therefore in this soil if we multiply the number of silt particles (obtained by counting) by 38, the result will be to the number of clay particles as the weight of the silt is to the weight of the clay. In 26 soils of widely varying texture, lately analyzed by the counting method and checked by the centrifugal analysis, an average of the factors was 35. The factors varied, however, widely enough to pre-

[1] Published by permission of the Secretary of Agriculture.

[2] Ann. Rep., Conn. Expt. Sta., 1886, p. 141; 1887, p. 144; 1888, p. 154.

[3] Ann. Rep., Cal. Expt. Sta., 1891–92, p. 243.

[4] Proc. Asso. Off. Agrl. Chem., Bull. 56, Division of Chemistry, U. S. Dept. of Agr., 1898, p. 67.

[5] U. S. Dept. of Agr., Bureau of Soils, Bull. 24.

[6] Bulletin No. 89, Utah Expt. Sta., 1904.

[7] Verhand. d. II. Inter. Agrogeol. Konf. Stockholm, 1910.

clude using this method as a regular routine analysis for all soils.

There are several reasons explaining the divergence of these factors from the average. To obtain one factor which would obtain for all soils, the silt particles (and likewise the clay) would have to be of a uniform size, shape and weight. These conditions do not exist in nature. However, if one size graded into the next, and the shape and specific gravity of the material were fairly uniform, the factors would still not be widely divergent. Another difficulty enters here; not only is this hypothesis doubtful, but it is not possible, on account of the great difference in size of the largest silt particles and the smallest clay particles, to have a counting plate and objective so calibrated that the very small clay particles could be counted and at the same time to give a field large enough to include a number of silt particles. So in order to get the silt it was necessary to use a low-power objective, and consequently many of the smaller clay particles were not counted.

The disadvantages of the method are that it is not applicable in every instance because of the small mass of clay particles in some soils; that the counts are not sufficiently uniform where great accuracy is necessary; and that in order to get the best results the operator must have a large experience in the usual methods of analysis.

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On a Posterior Communication of the Air-bladder with the Exterior in Fishes

Recently, while examining the visceral anatomy of *Ophiocephalus,* my attention was called to what appeared to be a posterior communication of the air-bladder with the exterior. My species is *Ophiocephalus maculatus,* and before its death had been an aquarium pet of a Chinese in Redwood, California. Hence from whence it came is not known, but doubtless from somewhere in China, for many of this singularly hardy fish are carried alive by the Chinese from their home country.

The abdominal cavity of *Ophiocephalus* extends for a greater distance behind the anal opening than in front of it for the accommodation of the very long air-bladder, which reaches almost to the base of the caudal fin. About midway in the length of the air-bladder a wide tube of thin membrane is attached, which opening at its lower end to the exterior through the genital pore, appears at first sight to be a duct from the air-bladder, more especially as it is of the same white, glistening membrane. It, however, ends blindly against the wall of the air-bladder where it is so firmly attached by the incasing fibers of the latter extending over and around it that considerable tearing is necessary to detach it. Both the tube and the air-bladder, where the tube joins, were opened and examined under the microscope but no opening was found in either. The end of the tube is round and blunt. Into its lower end the vas deferens opens. Just above where the vas deferens enters, or just below the middle of its length, the tube expands into a large triangular pocket with a blunt point directed forward.

Almost a century ago Weber ("De aure et audita Hominis et animalium," Leipzig, 1820, p. 73) described in *Clupea harangus* a communication from the posterior end of the air-bladder with the exterior through a duct opening with the vas deferens into the genital pore. This condition being so nearly parallel with that described above for *Ophiocephalus* leads me to question whether the tube described by Weber was not also a blind tube, and not actually opening into the air-bladder.

Having no specimens of *Clupea harangus,* and having other problems on hand, I have not attempted to go more deeply into this subject. As the supposed fact that *Clupea harangus* has a posterior opening to the air-bladder has been repeated several times since the time of Weber without any one attempting to verify his work (or at least indicating that he has verified it), it is desirable that some one do so. I pass the problem on for what it may be worth.

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